Evolution and Social Theory: The Problem Of Culture.

Keith Sharp

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ABSTRACT

The primary aim of this thesis is to establish that the central tenets of conventional social theory cannot be sustained in the light of modern evolutionary biological theory (the theory of inclusive fitness). In particular, it is argued that the central social scientific assumption of a radical separation between biology and culture raises insuperable problems for the formulation of the motivation of action, when the logical consequences of modern Darwinian biology are fully considered. At the same time, however, it is argued that recent attempts to apply evolutionary theory to the direct analysis of human social behaviour - *human sociobiology* - have been fundamentally unsuccessful, theoretically, but above all, empirically.

The central problem which the thesis formulates, therefore, is of how to conceptualize human action as motivated in accordance with the expectations of evolutionary biology, whilst recognizing that such action does not necessarily conform either in its immediate subjectivity, or in its objective distal consequences, to the predicted patterns of inclusive fitness theory. The solution to this problem is sought through an analysis of the level of phenotypic selection at which explanations should proceed; it is concluded from this that the appropriate level must be that of *psychological mechanism*, and that the sociobiological emphasis on overt behavioural pattern crucially ignores the interactive nature of the gene-environment relationship. Accordingly, it is argued that only by proceeding at the level of psychological mechanism, can the motivation of culture in general, and in particular maladaptive behaviour, be understood in terms of evolutionary theory.

Through an examination of the evolutionary logic of psychological models, it is argued that evolutionary theory strongly suggests a model which resembles, in important respects, that advanced by classical psychoanalytic theory. In particular, it is argued that the psychoanalytic conception of motivation, and the special relationship which Freud conceived between instincts and objects permits an analysis of empirical behavioural variation - even *maladaptive* variation - in full accordance with the expectations of evolutionary theory. The final chapters of the thesis illustrate the method of analysis proposed with reference to the example of apparently maladaptive variation in human sexual behaviour.

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K.S., Edinburgh, August 1990.

I declare that the contents of this thesis are entirely my own work.

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INTRODUCTION

> It is the possession of a common culture and the ability to communicate and pass it on to others that distinguishes the human being from other animals. Humans are human because they share with others a common culture, a culture which includes not only the artefacts of its living members but also those of members of past generations. This is the heritage awaiting those as yet unborn. Human beings are able to develop and pass on their culture by means of language, which is, of course, itself a product of culture. Language has to be learned in the same way as other elements of a culture and once this has been accomplished, the individual can acquire the rest of his or her culture (Bilton et al, 1981, p.10).

This passage, taken from a popular introductory text in sociology, is a telling summary of some of the most fundamental assumptions in modern social theory. This is a thesis about these assumptions, and specifically the implications for them of modern evolutionary biology. My central purpose is to demonstrate that the validity of such assumptions is now irrevocably damaged by modern evolutionary insights. At times, readers familiar with the received wisdom of the social sciences will find it difficult to believe that it has been written by someone who has been trained, first and foremost, as a sociologist, and who is employed to teach that subject. The conclusions which I subsequently reach are, after all, effectively a catalogue of what are generally accepted to be grave sociological vices: the "naturalistic fallacy", essentialism, psychologism and so on. Yet vices though they may be, I hope I demonstrate in the following pages that the time has now come for social

scientists to seriously re-evaluate them in the light of the very impressive new evidence provided by evolutionary theory.

Because this thesis has been written by a sociologist (who, despite appearances, still uses that name), principally for other sociologists and closely related disciplines, I have attempted to keep the biological discussions as free from technical detail as possible. Furthermore, because the subject of evolutionary biology is one of which most social scientists are proud to remain ignorant, it has been necessary to devote a part of chapter one to a fairly straightforward explication of the state of modern evolutionary biology. I have concentrated on general principles only, as it is a misunderstanding of these which, I believe, has led to both the unjustifiable rejection of evolutionary biology by conventional social scientists, and its mis-use by many "sociobiologists". In addition to this basic exigesis, however, I have attempted to address a number of criticisms levelled at evolutionary theory by both non-biological critics, but also by a number of leading biologists.

In the second chapter, I have advanced a critique of the most prominent attempts so far to utilise evolutionary theory in the understanding of human social behaviour: that is, *human sociobiology*. Whilst it is argued that the tendency of social scientists to reject sociobiological explanations - such as those put forward by E.O.Wilson (1975, 1978) and Lumsden & Wilson (1980, 1983) - on the grounds of their imagined political implications is untenable, it is agreed that there are certain empirical objections to sociobiology, which in fact raise an insurmountable obstacle to the approach's successful application. In particular, it is argued that the

insistence of sociobiologists to determine any particular trait's current adaptive value, prevents the empirical recognition of either behavioural variation or maladaptive behaviour.

The third chapter examines the alternative position advanced by "conventional" social theorists. I am aware that some readers may object to my grouping so many diverse traditions under this heading, and of course for some purposes this objection is quite legitimate. Nevertheless, for the purposes of my argument the grouping is justifiable, since I am concerned here only with the extent to which various theoretical models incorporate biology into their explanations of human behaviour. In this respect, it can be seen that even apparently diverse theoretical positions share a common disregard for biological factors in their analyses. It is argued that this disregard, whilst considered self-evidently justifiable by most social scientists, cannot be sustained when the central insights of modern evolutionary theory are considered. It is therefore the aim of this chapter to establish that whilst social scientific explanations of human behaviour do have certain empirical advantages over the crude sociobiological approaches discussed in chapter two, their reliance on the biology-independence of the source of human motivation renders them ultimately untenable. Chapter three thus concludes with a paradox: if approaches to human behaviour which adopt an evolutionary biological stance are unable to explain critical empirical realities, but that non-biological alternatives are theoretically untenable, how exactly can human social behaviour in all its variety in fact be explained?

The following chapter attempts to resolve this paradox by considering the possibility that biologically determined traits could be explained as the *products* of natural selection whilst displaying no current adaptive value. It is argued that this could occur as the result of evolutionary *time-lags*; that is, differences between the environment in which the trait in question evolved and that in which it now develops, which produce different phenotypic outcomes. This principle is then applied to the problem of human behavioural variation and maladaptive behaviour, and it is suggested that only by focussing on the *motivation* of action can these difficulties be resolved. Before moving on to a precise examination of the model of motivation which this analysis implies, however, the question of the nature of mind which is suggested is addressed, and the argument made for taking what many may regard as the potentially unfruitful decision to reduce analysis to this level.

Once I have established that this level of analysis is indeed legitimate and fruitful, the following three chapters are devoted to the elaboration of the implications of evolutionary theory for the mind, motivation and the meaning of action. Chapter five is principally concerned with the relevance of the model of mind, and theory of motivation offered by classical (that is, orthodox Freudian) psychoanalytic theory, and reaches the conclusion, following an examination of key environmentalist modifications of this position, that Freud's original formulation of these issues has considerable significance in the present context. In particular, it is argued that Freud's often missed, but nonetheless crucial, emphasis upon the deep, phylogenetically determined content of the mind and its processes, together with his fundamentally individualistic conception of

motivation are uniquely relevant to the theoretical problems outlined earlier.

This observation is further explored in the chapter which follows through an examination of the general question of human consciousness and its relationship with the evolutionary process. I am well aware that the evolutionary analysis of consciousness and perhaps especially *self*-consciousness is a controversial undertaking: after all, it is precisely the supposed uniqueness of human consciousness which is generally held up as the chief justification for the assumption of the biology-independence of human motivation. Nevertheless, I have attempted to demonstrate that despite the wide acceptance which this position has gained, it is impossible to maintain in view of modern evolutionary theory. This is because, it is argued, to acknowledge that a human capacity has an organic base - as it must, by definition - then one must also acknowledge that the characteristics of the capacity in question were shaped by the normal processes of organic evolution. Thus, far from providing the ultimate justification for the removal of human behaviour from the biological sphere, the existence of human consciousness must in fact itself be subjected to evolutionary biological analysis. In chapter six, therefore, the evolutionary significance of consciousness is examined, and it is concluded that concsiousness, far from providing the ultimate data of motivational reality, is in fact only explicable as part of a *self-deceptive* mechanism, which serves, much as classical psychoanalytic theory always maintained, to exclude certain motivational realities from awareness.

The argument developed in chapter six, however, does raise an important problem: whilst it is quite explicable in evolutionary terms for an actor to deceive themself about their own motives - in the interests of better deceiving others - it remains to be explained why actors should on occasions evidently deceive themselves about the motives of others. In other words, the argument is faced with yet another formulation of the "problem of altruism". If culture, as social scientists use the term, may be loosely defined as a collective consciousness, or *self*-consciousness, then how can a theory based upon individual genetic interest explain the motivated acceptance of other unrelated actors' deceptions? The solution which in chapter seven is offered to this problem returns to the fundamental principles of evolutionary biology outlined in chapter one, and relates to the phenomenon of kin altruism; that is, the evolution of altruism on the basis of genetic similarity. Since, however, modern manifestations of cultural identity do not correspond to actual genetic relatedness, this analysis also offers an important illustration of the effects of evolutionary time-lags, as discussed earlier.

The final two chapters offer an illustration of the main theoretical points made in earlier chapters through an examination of certain aspects of variation in human sexual objects and practices. I have chosen to use this particular example for a number of reasons. First of all, because this is an area of human behaviour which, from the point of view of evolutionary theory, one would expect to be most strictly under genetic control, but which in reality appears very frequently to exhibit characteristics which are entirely counter-adaptive. Secondly, this is an aspect of behaviour which is, empirically, relatively resistant to analysis of the conventional

sociological kind, and which therefore highlights, as clearly as possible, the weaknesses which I am suggesting exist with this form of analysis. Thirdly, although sexual behaviour *per se* can be seen as a relatively discreet aspect of human behaviour, the theory advanced here, which supports in many respects that advanced by Freud, recognizes the absolute centrality of sex to all forms of behaviour, since as I repeatedly stress, it is ultimately reproductive success, *and that alone,* which determines the success or failure of any genetically determined trait.

The analysis which, in the last two chapters, is offered of these phenomena is, of course, by no means exhaustive. Whilst it is my hope that what I have to say will contribute in some way to the debates which are currently taking place in this field, and perhaps especially to the whole question of the evolutionary psychology of sexual commitment and deception, it is primarily intended to illustrate some of the theoretical points made in earlier chapters. As chapter one makes clear, the details of any evolutionary analysis must inevitably often be speculative in nature, simply because reliable data upon the outcomes of particular genetically based adaptations in past environments will rarely be available. The crucial point is however, as I attempt to demonstrate, that whilst the details of the analyses offered here will (I hope) be the subject of further debate, they rest on a fundamentally sound theoretical base, and can thus make a legitimate claim to serious attention.

CHAPTER ONE

An Outline of Modern Evolutionary Biology

My aim in this chapter is to outline as clearly as possible those features of modern evolutionary biology which are fundamental to the argument I shall subsequently develop. I shall begin by considering certain rather general points about the nature of Darwinian evolution , and then move on to examine the specific formulation central to the modern theory - the theory of inclusive fitness, as proposed by W.D.Hamilton. In the course of this examination, I shall consider a number of objections which have been raised against both the specifics of Hamilton's model, and perhaps more importantly, against Darwinian evolutionary theorising in general. Although I shall have something to say about specifically biological issues, I shall avoid lengthy discussions about the more technical details of genetics, substituting them where possible with appropriate references. I shall instead be mainly concerned with a number of epistemological objections to evolutionary biology, and in particular the charges that it is necessarily tautological and hence in Popper's terms unfalsifiable, and related to this, that it is guilty of following what Lewontin (1978) calls an "adaptationist program". I shall conclude this chapter by considering the implications of these arguments for the form and status of evolutionary explanations.

Natural Selection: First Principles

It is helpful to begin this discussion of modern evolutionary biology with some rudimentary observations about the fundamental process of natural selection, as first proposed by Darwin, since not only is this central to the argument I shall develop in later chapters, but as I shall later show, its consequences have often been seriously misunderstood. Darwinian evolution is generally characterised as involving three distinct components (See Lewontin 1978). First is the *principle of variation*, that is, individuals within a species display differences from one another. Second is the *principle of heredity*, that is, the variations manifested are capable of biological transmission. Third is the *principle of natural selection*, which holds that given natural limits on resources, those individuals possessing variations which enable them best to survive and reproduce will leave more offspring than others; the most successful variations will thus come to dominate. As I shall show later in this chapter, it is this last component of the Darwinian theory which has been subject to most debate and disagreement, but for the present let me emphasise some basic points about the first two.

The first and second components, the principles of variations and heredity, necessitate an important distinction between the biological, heritable basis of a particular trait and the trait itself. In the language of modern biology, these are termed the *genotype* and *phenotype* respectively. The importance of this distinction lies in the fact that an observed variation may be the consequence of factors other than genetic variation, and thus it would not be heritable by biological means. An

obvious example of this would be variations in the height of plants, brought about by differential exposure to sunlight; systematically in-breeding the taller plants would not lead to a positive shift in the mean of the height distribution, because the taller plants, despite their observed variation, are genotypically identical to the shorter plants. Evolution, then, presupposes a linkage between genotype and phenotype, and it is because of the *effects* of the phenotype that the genotype is maintained.

A further point which needs to be made in this regard is the source of variation in the genotype on which selection acts. There are only two means by which variation can occur in the genotype: *mutation* and *recombination*. The first refers to randomly occuring failures of DNA to replicate exactly, with the effect that the genotype passed on to the offspring differs from that which would be expected under normal circumstances. In by far the majority of cases such mutations are deleterious; in the rare cases when mutations confer a positive phenotypic benefit to their bearers, they are selected. Recombination refers to the emergence of new genotypical forms which result from the reshuffling of chromosomes which normally occurs betwen generations. It is therefore the process by which most variation occurs, but the ultimate source of such variation remains mutation. Interestingly, it is the fact of recombination which provides the strongest answer to the apparent Darwinian paradox of the evolution of sexual reproduction. Sex seemed to be such a paradox because its effect is to reduce the chances of any heritable variation being passed on through the very process of recombination. Yet, since it also provides the source of much of the variation on which selection acts, it is now understood as a mechanism

with a very clear selective advantage (See Trivers 1985, Ch.13 for a summary of these arguments).

Perhaps the most obvious point to notice about the source of genetic variation - and one which is frequently misunderstood - is that it is entirely *random*, and therefore non-directional. The name "Lamarckism" is conventionally given to the opposing view,after the 19th Century biologist Lamarck, and generally involves a belief in the inheritance of acquired characteristics. For example, giraffes have elongated necks as a direct result of the stretching behaviour of their ancestors; or human beings are developing, and transmitting to their offspring, alternative morphologies as a consequence of their sedentary lifestyles. This view thus assumes that the genetic material which is transmitted through reproduction is capable of modification as a result of environmental changes acting on the *phenotype* for which it codes. Lamarckism is therefore entirely at odds with everything that is known about the mechanism by which genetic material is translated into cellular development and morphology; DNA is a strictly irreversible code. (For a clear account and refutation of recent attempts to ressurect a form of Lamarckism, see Dawkins 1982 pp.164-178; see Trivers 1985, Ch.5 for a basic summary of the genetic processes involved.)

Units of Selection: Groups, Individuals or Genes?

The final feature of the Darwinian explanation, the principle of natural selection, whilst at first sight perhaps uncontroversial, has provoked the greatest debate of all. The focus of this debate has been the question of

what should be taken as the primary unit of selection; that is, the basic unit for whose reproductive benefit selection acts. The principal contenders in this debate have been the the group or species, the individual organism and the individual gene. Although it is clear that Darwin himself assumed that selction occurred at the level of the individual (Darwin, 1859; see also Ruse 1989, pp.34-54), a number of theorists subsequently proposed that selction operates at the level of the group (most notably, perhaps, Wynne-Edwards, 1962). Clearly, the consequences of such a claim for the nature of evolution are far reaching : behaviour selected for its contribution to the reproductive success of the *group* to which the individual belongs will be different indeed from behaviour selected for its contribution to the reproductive success of the particular individual. In order to demonstrate the importance of this issue, it will be helpful briefly to summarize the thrust of the group-selectionist argument as represented by Wynne-Edwards.

Wynne -Edwards' central argument was that the principal survival problem facing any species is a tendency to reproduce beyond available resources. In response to this, he argues, mechanisms will be selected which enable individuals within the population to homeostatically control their own population densities - by means of reproductive restraint - in relation to variable food resources. Much animal behaviour was thus characterized by Wynne-Edwards as examples of *epideictic displays*; a term coined to mean behaviour which is adapted to give conspecifics an immediate estimate of local population density, and thus trigger reproductive behaviour appropriate for the survival of the population.

In this sense, Wynne-Edwards was advancing a theory which predicted the selection of reproductive altruism (defined in biology as any behaviour which enhances the reproductive success of another individual at the expense of the actor). His argument assumed that because the availability of food resources is something which affects the entire population, slection acts upon *collective* responses to the problem. He therefore suggested that because *extinction* is the greatest threat to a population, mechanisms which serve to avert this possibility will be selected, even if this is achieved at a cost - as would almost always be the case - to personal reproductive success. In other words, populations which evolved ways of preventing themselves from going extinct, by avoiding the chief cause of extinction - that is, reproduction beyond available resources - would survive and reproduce more effectively than those lacking such adaptations. In this way, selection is held to operate at the level of the group.

At first sight Wynne-Edwards' theory looks convincing. As he shows, there are many examples of such apparent reproductive altruism amongst animals, and perhaps not least, amongst human beings. Nevertheless, following the publication of Wynne-Edwards' work, an enormous amount of theoretical (and empirical) criticism emerged, which lay bare the fundamental flaws underlying his theory. One of the chief oponents to Wynne-Edwards was the ecologist David Lack, who, using the example of clutch size in birds, sought to demonstrate that such apparent altruism was quite explicable from the point of view of individual selection (Lack, 1966). Lack makes the point that for any species of bird, there must exist

an optimal clutch size, that is, the number of eggs which the female bird needs to lay to yield the maximum number of offspring. Clearly, since the resources available to individual mothers is limited by ecological circumstances and carrying capacity , there will be individual reproductive costs in the form of inadequately provisioned offspring associated with excessive egg production. Reproductive restraints, therefore, may be quite explicable in terms of the benefits conferred to individual reproductive success by only hatching the number of offspring which available resources can support. In terms of selection, therefore, it can be seen that *individuals* who possess the means to maximize the number of survivng offspring, will leave more copies of this trait in future generations than individuals who possess traits which cause them to reproduce at a lower rate than is ecologically feasible.

It is worth briefly considering the conditions which would be necessary for selection to operate at the level of the group. Essentially, for group selection to occur, the rate at which groups go extinct would have to be comparable to that of individuals within groups, so that groups containing only altruists reproduce at a faster rate than those lacking altruists. Yet for this condition to be satisfied, it needs to be assumed that individuals within a group whose numbers were approaching extinction level, could not be selected to migrate to areas of lower population density, before extinction occurred. Thus, the theory would require the presence of homogeneously altruistic groups as a starting point, and not a consequence of selection. The problem with such an approach is obvious: those individuals within the group who are most adept at selfishly exploiting available resources to maximise their own reproduction - and

one such strategy could certainly be migration - will reproduce faster, and come to dominate the group (see Wilson, 1975 for an account of models of group selection).

It would not be an exaggeration, however, to say that the definitive solution to the level of selection argument came with the publication in 1964, of William Hamilton's papers "The Genetical Evolution of Social Behaviour" (Hamilton,1964a,b; see also Trivers 1985, Dawkins 1989, 1982, 1986. Barash 1977). The central contribution made by Hamilton was to focus attention on the role of genes in the selection process. In particular, he pointed out that since genes are the basic units of heritable material, genes which promote their bearers to behave in such a way as to increase the number of copies of themselves in future generations would be selected. From this he reasoned that since related individuals share genes with calculable probabilities, selection would favour genes which promoted not merely the survival and reproduction of their individual bearers, but also that of relatives who also possess the gene in question. This extended notion of individual fitness became known as "inclusive fitness".

The most obvious implication of Hamilton's insight concerned the circumstances under which altruism could be selected. Thus, a gene promoting an altruistic action towards an unrelated individual would suffer a net disadvantage, because the reproductive advantage conferred to that individual would not result in copies of the gene in question being transmitted to future generations. A gene, on the other hand, which promoted an altruistic action towards a relative who had a 50% probability

of also possessing that gene would enjoy a net advantage, but only when the cost of the altruistic act to the actor equals less that half the benefit it confers to the recipient. This situation admits of representation by the formula:

$$Br>C$$

where, B is the benefit to the recipient, C is the cost to the altruist, and r is the degree of relatedness between altruist and recipient.

Hamilton's model is therefore based upon the principle that the basic unit of selection is not the group, or even the individual organism, but the *gene*. The fundamental reason why genes should be treated as the basic unit of natural selection is that unlike individual organisms, and the groups in which they sometimes live, genes possess the capacity to make faithful replicas of themselves. As Richard Dawkins has so clearly expressed it (Dawkins, 1982, 1989), the organisms in which genes are present, are merely temporary "vehicles" which facilitate the process of replication[1]: unlike individual organisms, genes have the potential to be immortal. Their immortality or otherwise will of course depend upon the way in which they cause their vehicles to *behave*, but the vehicles themeselves could never be the unit of selection, for the very simple reason that they are always mortal.

It is with the foregoing observations in mind, that I wish now to turn to some of the criticisms which have been made of the approach I have outlined.

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[1]Unfortunately, as we shall see in later chapters, Dawkins' vivid mataphor leads him into a serious theoretical error when dealing with culture.

Function, Teleology and Tautology in Darwinian Explanations

As the summary of the modern evolutionary theory which I have presented should have made clear, evolutionary explanations derive their force from the consequences which the phenomenon in question has for the spread of the genes which determine it. In this respect evolutionary explanations are quite explicitly functional in structure: phenotype x exists in a species because it has the *effect* of making relatively more copies of the genes which code for it than other phenotypes. It is important, however, to distinguish this type of reasoning from the various illegitimate uses to which teleological explanation is sometimes put.

Explanantions are generally considered to be teleological when the cause of an action or event is given in terms of its consequences (see Cohen 1968, p. 47). Thus, to say that a man crosses a road in order to reach the other side is a teleological explanation, because the consequence of the action - reaching the other side - is here given as a cause.[2] In this case the explanation is quite admissable because of the legitimate imputation of purpose to the actor. Yet if the purpose of the actor could be shown to be unconnected with the consequences of his actions such a mode of explanation is not acceptable. Thus, if a man, aiming towards the the goal of reaching the opposite side of a road, is struck by a piece of falling masonry *which he could not have foreseen,* it would be obviously

[2]Strictly speaking, it is the aim of reaching the other side which causes his action, but since we can assume that the actor believes his aim will be realized, the distinction can be ignored for these purposes.

ludicrous to give the consequence of being struck by masonry as the cause of his crossing the road.[3]

Teleology, then, in evolutionary explanations is quite legitimate, because it is quite demonstrable that genes survive as a direct result of the *consequences* their actions have for their survival. The difference between this and the sort of conscious teleology or purpose which is usually attributed to human actors, is captured by the distinction made by biologists between *proximate* and *ultimate* causation. Ultimate causes are always the same: the phenomenon in question exists because of its relatively superior ability to reproduce the genes responsible for it. Proximate causes, however, are the ontogenetic means by which ultimate causes are realised. Thus, for example, the proximate cause of the human eye is the complex series of biochemical stages which runs from DNA to RNA to proteins to embryological development and so on. The ultimate cause of the eye, however, is the contribution it made, during its evolution, to the inclusive fitness of its bearers.

In this way, ultimate causation is always teleological; the proximate cause, however, may or may not involve teleology. For instance, in evolutionary terms, the teleology of human action is not an ultimate explanation for why a man crosses the road. Whilst it certainly gives a description of the immediate mechanism which led to the action, the ability to act teleologically (and, as I shall argue later on, the content of such an action) must itself be explained in ultimate - that is, teleological -

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[3]I add the clause "which he could not have foreseen" to eliminate the possibility that he could have been acting with an unconscious purpose: the significance of this will become clear much later on.

terms. It is because of the tendency to confuse the teleology which is inevitable in evolutionary explanations with purposive human conduct, that some biologists prefer to distinguish the two by using the term "teleonomy" to refer to the evolutionary process (Pittendrigh, 1958; see also Williams, 1966).

Whilst it is clear that such teleological (or teleonomical) reasoning is a logically admissable, and indeed necessary, feature of evolutionary explanations, it has led to a number criticisms centring around the charge that such explanations are, because of their teleological nature, unacceptably circular. I shall deal here with two main variants of this charge; first that evolutionary explanations are unscientific, because they are untestable, or, in Popper's terms, "unfalsifiable" (Popper, 1962), and secondly that the teleological reasoning involved leads inevitably to an "adaptationist program"(Lewontin 1978; Gould & Lewontin, 1979), in which the *a priori* necessity to assume an adaptive function for observed traits results in an absurd "Panglossian optimism", identifying adaptive purpose where none exists.

The claim that evolutionary biology cannot be considered scientific because it is formally unfalsifiable, can be disposed of with relative ease. It is clear that a phrase such as Spencer's "survival of the fittest", where "the fittest" are defined as "those who survive" is an unfalsifiable tautology for the obvious reason that by defining its terms as equivalent, the statement becomes by definition true. Yet there is nothing about the theory which this phrase is sometimes held to describe that is itself tautological. At its simplest, modern evolutionary biology states that genes which produce

phenotypes which have the effect of producing relatively more copies of themselves than others, will come to dominate. Phrased in this way, the theory certainly *appears* to be an unfalsifiable tautology. Since the mere existence of any trait is hereby considered to be adequate evidence for its relative evolutionary adaptedness, no trait could, in principle, be found which would lead to the rejection of the hypothesis that all traits are selected because of their relative evolutionary adaptedness. Yet this only holds if it is accepted that the only form in which the theory can be tested is that given above. Plainly this is not so.

In fact this way of expressing the theory is little more than a summary of the conclusions of a series of antecedent stages of reasoning, each of which is perfectly amenable to falsification. Thus, once it is established that populations exhibit genetic variability, that this variability is ultimately the product of random mutation, that genes are replicators, and finally that genes are systematically linked to the production of phenotypes, the evolutionary hypothesis stated above follows by simple deductive logic. Quite clearly, were any of these falsifiable conjectures actually falsified, then so, deductively, would be the entire evolutionary theory. But since none have been, despite rigorous attempts to do so, we must conclude that the modern evolutionary theory is far indeed from being unfalsifiable; it merely remains unfalsified.

The claim that evolutionary biology is guilty of "Panglossian optimism", or an unwarranted adaptationism is more complex, but does not seem to pose the difficulties which critics such as Lewontin and Gould (1979; Lewontin 1978) imagine. Essentially this argument holds that like

Voltaire's Dr Pangloss,who considered that the nose is the shape it is so that one can rest one's spectacles on it, modern evolutionary biologists are so preoccupied with the notion of adaptation as the only cause of evolution, that they relentlessly attribute often quite spurious adaptive significances to traits which simply have no significance. I have already argued that there is nothing logically wrong with assuming that observed traits exist by virtue of the purpose they serve, and that such an assumption can be legitimately deduced from a series of strictly falsifiable biological premises. However, against this, a minority of biologists have suggested various processes by which traits may come to exist *independently* of their adaptive significance.

The first of these which I shall consider, is the possibility of mutations occurring which have no consequences for inclusive fitness, of either a positive or a negative kind (see Dawkins 1982 p.32 for a discussion, and distinction from a rather different and, for these purposes, unrelated debate in molecular genetics). Clearly such "neutral" mutations could not be selected by the process assumed by evolutionary theory, as they confer no adaptive benefits to their bearers. Similarly, however, they could not be removed from the gene-pool by this process, since they do not harm their own chances of survival: they are simply neutral as regards selective pressures. The problem with this argument is that whilst it is theoretically possible for a trait to occur in this way, in practice its consequences must always be negligible. This is because, as mathematical models of evolution have shown, even "obviously" insignificant traits, magnified over evolutionary time, can confer decisive benefits to the inclusive fitness of their bearers (Haldane 1932). Similarly, even if such a

trait could be identified, it would have by definition to be so insignificant to the overall physiology and behaviour of the organism, that it would almost certainly not warrant any detailed analysis.

A second supposedly non-adaptive path to the fixation of a trait, is the existence of "pleiotropy". Whilst evolutionary biologists, in the interests of clarity, generally speak of genes as though they exerted a single effect upon a single trait, the reality is more complex. In fact, genes are usually pleiotropic, that is, influence more than one phenotypic effect. The principal difficulty for adaptive explanations which is raised in this connection, is the possibility that any observed trait may in fact be a deleterious side-effect of pleiotropy. In other words, the trait may exist purely because it is genetically linked to another adaptive trait, and so any adaptive "significance " which the adaptationist finds for it must be spurious (Lewontin, 1979).

Again, this path to fixation is theoretically possible, but could only be considered empirically significant if certain features of the relationship between genes and their phenotypic effects are ignored. A gene does not produce a phenotypic effect in isolation. Rather, it does so in an environment of other genes, all of which affect the embryological context of its development. Since this context is obviously vital for the development of any phenotypic effect - genes for eyes are of little value without also genes for eye-sockets - selection will strongly favour genes which promote adaptive phenotypic effects of *other genes* if these are necessary for their own survival. One mechanism by which this process occurs is the selection of "modifier" genes, which have the effect of

modifying the phenotypic effects of other genes, such that the deleterious pleiotropic side effects of an adaptive phenotype can be either reduced, or decoupled altogether (see Fisher 1930 for an early statement of this principle; see Dawkins 1982, Ch.8 for a more detailed discussion of its relevance to modern evolutionary theory). In this way, pleiotropy could only represent a significant path to the existence of non-adaptive traits if it could be assumed that such modifications in phenotypic effects by genes at other loci could not occur.

Whilst alternative, non-adaptive paths to trait fixation seem unlikely to be significant, there are a number of more pertinent issues which the charge of adaptationism raises. These are what Richard Dawkins calls "constraints on perfection" (Dawkins 1982, Ch.3), that is, reasons why evolutionary biologists should be wary of assuming that any observed trait represents the optimim solution to the adaptive problem in question. In fact, I shall argue in subsequent chapters that it precisely a failure to recognize one of these constraints - namely the effect of "time lags" (Dawkins 1982 p.35) - which has made most attempts to apply evolutionary biology to human behaviour fundamentally unsatisfactory.

Time is an important constraint on perfection because ecological time and evolutionary time are not necessarily the same. Whilst traits take many generations to evolve and become fixed, the environments in which they operate can undergo rapid change in very short time periods, even within single generations. This means that an organism can, in theory, exhibit traits which under current environmental conditions do not maximise its inclusive fitness, because the trait in question evolved to maximise fitness

under quite different circumstrances (see Maynard Smith, 1976 for an attempt to quantify such effects).

Whilst, as Dawkins points out (Dawkins 1982 p.35), this reasoning can provoke accusations of invoking *post hoc* rationalizations for the failure of an hypothesis to resist falsification, such accusations rather miss the point. The principal reason for this is that the isolation of a trait which is not adaptive is not sufficient grounds to reject the evolutionary hypothesis. This is for the reason advanced above, that the "evolutionary hypothesis" is not an hypothesis at all, but a conclusion logically deduced from a series of other unfalsified hypotheses. Thus, by the same reasoning, the acceptability of the claim that an observed trait is not adaptive under current circumstances, rests entirely upon the adequacy of the premise that evolutionary change does not necessarily occur in parallel with ecological change. Since this premise is not in doubt, there is no good reason to object to the time lag explanation. Indeed, time constraints such as this should be recognized as an essential feature of any coherent evolutionary explanation.

Another compelling reason why it would be foolish to expect evolution to produce adaptive perfection is the constraint imposed by available genetic mutations. Clearly the basic prerequisite for the selection of any trait is the *emergence* of that trait, which we can assume is a random affair. For this reason, some critics have attacked evolutionary explanations because they run the risk of attributing the absence of certain characteristics in organisms to their adaptive liability, rather than to the fact that appropriate mutations never occurred. As Lewontin (1979) has put it:

"Thus, although I might argue that the possession of wings in addition to arms and legs might be advantageous to some vertebrates, none has ever evolved a third pair of appendages, presumably because the genetic variation has never been available."

This is certianly one possible explanation for the absence of wings in some species, but has no more *a priori* force than the hypothetical adaptationist argument that some animals never evolved wings because they did not confer any selective advantage. More basically, however, it needs to be emphasised that the "lack of available variation" argument could only explain the absence of certain characteristics. All it means in the case of characteristics which do exist is that they must have conferred a selective advantage on their bearers, *relative to other available variations*. This is certainly no challenge to evolutionary biology; indeed to argue the reverse, that a species had reached a state of adaptive perfection, would involve the implicit denial of the very evolutionary process which produced this situation.

One final point which needs to be made in relation to constraints upon adaptive perfection is that evolution generally acts, by necessity, upon mutations which produce relatively minor phenotypic improvements in the organism. This means that any evolutionary change is necessarily constrained by those which have preceded it to produce the organism in its pre-mutant form. Considered in this light, far from expecting adaptive perfection, one may well wonder why animals are not, as Dawkins has put it "risible monstrosities of lashed-up improvisation, top-heavy with grotesque relics of patched-over antiquity" (Dawkins 1982, p.40).That they

are not is obvious. But it is also important, since it gives a clear indication of the degree of perfection which we are justified in expecting, and furthermore, is a graphic illustration of the degree of available mutation which it is reasonable to expect.

The Scope and Limits of Evolutionary Biology

I have tried, in this chapter, to give an outline of the basic theoretical premises of modern evolutionary biology, and have considered a number of arguments which, it has been claimed, limit its accuracy and explanatory value. I wish now to conclude this discussion by summarizing the implications of this for the nature of evolutionary explanations, and in particular those which I shall develop in later chapters.

I think it is now beyond dispute that the existence of biologically based traits is ultimately explicable only in terms of the contribution they make to the inclusive fitness of their bearers. Unfortunately, it is equally clear that this fact raises a number of difficulties for the formulation of evolutionary explanations. The first of these is the "time lag" question which I have discussed above. Since adaptations will very often have been selected for the advantages they confer in an environment different from that in which they currently exist, the certainty with which particular significances can be ascribed to currently observed traits will necessarily be restricted. This problem is of an essentially empirical nature, since its seriousness rests entirely upon the quality of data available for the particular environment in question. The difficulty is

especially acute, however, in the human case, where it is virtually impossible to be certain about the exact conditions for which selection took place. Nevertheless, as I shall later show, there now exists sufficient evidence of various kinds to considerably increase our confidence in certain general features of this environment.

Related to the problem of certainty about the environment of evolutionary adaptedness, is the difficulty which time lags pose for the testability of suggested adaptive functions. In the event that we could be certain that the current environment is that for which a particular trait was selected, evolutionary functions could in principle be tested, by , for example, making predictions about the relative reproductive success of those individuals possessing the trait. Since, however, the trait in question could be the result of an adaptation for a quite different environment, such testing would yield unreliable results. In fact, it is a central strand of the argument developed in subsequent chapters that much contemporary human behaviour is the result of just such circumstances.

What all of this means, then, for evolutionary explanations - and especially those concerned with human behaviour - is that a degree of ultimate uncertainty is inevitable, and, given the nature of the constraints upon certainty I have outlined, must surely be desirable. This is far indeed, however, from advocating random speculation. Rather, it suggests that a recognition of the limits to our certainty should lead us to a methodology which replaces groundless dogmatism with a plausible coherence with that which is certain.

It is with these observations in mind that I now wish to turn to the evolutionary explanation of human behaviour, and first of all to the emergence of human "sociobiology".

CHAPTER TWO

The Sociobiology of Human Culture.

The second half of the 1970s saw a major growth in attempts to apply the principles of evolutionary biology to the explanation of human social behaviour. Perhaps the single most important catalyst in this development was the publication in 1975 of E.O.Wilson's massive, Sociobiology: The New Synthesis (Wilson 1975). This was followed by a series of attempts to utilise the central theme of this work - that human social behaviour is explicable in terms of the central theorem in evolutionary biology - and develop a thorough and sophisticated biological understanding of human culture. My aim in this chapter is to consider the success of this enterprise, and to raise some of the problems which it poses. Before progressing to this, however, it is important to emphasise my reasons for considering only the *explanatory* success of human sociobiology, and to distinguish my analysis from the politically motivated attacks which followed the publication of Wilson's work.

The years following the publication of Wilson's book saw a number of attempts to discredit the enterprise of sociobiology by equating it with a wholesale endorsement of everything critics perceived as wrong with the status quo. One reviewer characterised sociobiology as:

> The latest attempt to reinvigorate these tired theories [which] provided an important basis for the enactment of sterilization laws and the restrictive immigration laws by the United States between 1910 and 1930 and also for the eugenics policies which led to the establishment of gas chambers in Nazi Germany (Allen et al,

in Caplan 1978, p.260).

Similarly, it was claimed that sociobiology is nothing more than a reflection of the dominant Western capitalist ideology, devoid of any serious scientific merit or purpose (see for example Sahlins 1977). It is my contention that these arguments are not merely completely irrelevant to an assessment of the explanatory claims of sociobiology, but that if taken seriously, they deny in principle that such an assessment is possible.

The first point to be addressed in this connection, is the relationship between a scientific theory, no matter how ludicrous, and the actions which are performed on its basis. Unfashionable though such a point of view may be, it is undeniable that there exists a logical gulf between the claims of a theory and the policies which are enacted on the basis of such claims (see Weber 1949). Scientific theories figure in human actions only insofar as all actions are grounded in relevant assumptions about the nature of reality, and any given scientific theory constitutes such an assumption. In this way, scientific theories are instrumental in the formulation of policies in that they provide a means to predict the outcomes of proposed courses of action, and can thus predict the efficacy of a given policy in, and even suggest alternative policies which would be more efficacious in, reaching a given aim. To illustrate this we could take two hypothetical policy-makers who wish to devise a policy to promote social cohesion. Let us also suppose that a sociobiological hypothesis that human beings are more likely to be altruistic towards individuals who resemble themselves is true. The policy-maker who accepts this hypothesis, and builds his policy on this assumption will obviously have

greater success than the one who ignores it as mere ideology. The theory does not dictate what is desirable - by definition - it merely alerts the policy-maker to the most efficacious means to achieve the desired goal. Science can thus impinge upon policy decisions only insofar as it determines what is *possible*, and in this it can be either right or wrong.

There is however a more serious objection to those who reject theories on ideological rather than scientific grounds, which has to do with the problem inherent in all forms of epistemological relativism. It has been claimed, for example by Marshall Sahlins (1977), that sociobiology is merely a reflection of the ideology of advanced laissez-faire capitalism. Similarly an article by the "Sociobiology Study Group of Science for the People" (reprinted in Caplan 1978) claims that Wilson's Sociobiology is a "manifesto of a new, more complex, version of biological determinism"(p.281) and that such theories "all describe a particular model of society which corresponds to the socioeconomic prejuduces of the writer" (p.280).

Unfortunately, these objections are irreconcilably incoherent. The reason for this is that they are not grounded in a principled distinction between theories which are mere ideological reflections, and those which are correct. In fact they are little more than a crude popularization of the sort of argument found, for example, in the work of Paul Feyerabend (1975), in which the possibility of culture transcending truth is in principle denied. The objection to this is well-known: if no statement can claim any more objective truth than any other, then this proposition must itself be rejectable on these grounds. Thus, in the present case, for the claim that

sociobiological theories are mere cultural artifacts to be true, it would also have to be true that *this claim is a mere cultural artifact*, and hence, in principle, no truer than the sociobiological claims which it seeks to dismiss.

When one examines the grounds on which sociobiological theories *are* distinguished from these claims about them, a rather extraordinary conclusion emerges. If it is claimed, as in the quotation above, that sociobiological theories merely "correspond to the socioeconomic prejudices of the writer", then in the absence of any other distinguishing grounds, we must also assume that the same is true of claims *about* sociobiological theories. All that is thereby left, however, to distinguish the two statements is a basically aesthetic judgement about the socioeconomic prejudices underlying each. And since aesthetic judgements must also be the product of socioeconomic prejudice - because on this view science and aesthetic judgement are the same - they cannot simultaneously be grounds for assessing the impact of such prejudice.

None of this is to deny that theories, including sociobiology, may yield correct information which many find unpalatable, and which many would prefer was not the case. But this is no more true of sociobiology than it is of medical oncology. Similarly, it does not deny that a factual understanding of the reality of a particular phenomenon may enable individuals so minded to successfully perform acts of unimaginable destruction and harm. Again, this is no more the case for sociobiological

theories than for any other.[1] Finally, it does not deny that incorrect theories can be utilized in policy-making of both desirable and undesirable kinds, and that great harm can come of the application of incorrectly understood principles. This. once more, is not special to sociobiologists, but rather highlights the importance of the rigorous assessment of all scientific claims, and the real harm which can result from the denial of this enterprise.

The Claims of Human Sociobiology

It is my intention in this chapter to offer a critical examination of the enterprise of explaining the contents of human culture in ultimately adaptive terms. I shall not, therefore, be concerned for the time being with the work of those biologists - for example Dawkins (1982, 1989), and Boyd & Richerson (1985) - who have sought to explain human culture in terms independent of its biological foundations; the underlying assumptions of this approach do not differ from those of conventional sociology and anthropology, which will be examined in the next chapter. Instead I wish to illustrate the difficulties involved in the formulation of a sociobiology of human culture with reference to the work of P.L.van den Berghe, on human families (1979), and especially to the work of E.O.Wilson and C.J.Lumsden (1980, 1983), which is perhaps the most thorough and sophisticated attempt to offer an ultimately adaptive account of human culture, and especially cultural change, to date.

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[1] It is instructive that Noam Chomsky has pointed out that extreme environmentalism, if an accurate theory of human behaviour, "removes all barriers to coercian and manipulation by the powerful" (Chomsky, 1975, p.132)

The most obvious and perplexing fact which has distinguished the study of human behviour from that of all other species, is the human capacity for behavioural variability. It is quite clear that any attempt at explaining human social behaviour must take the explanation of this variability as among its central tasks, and in this, evolutionary biology is no exception. But evolutionary biology faces an additional problem: not only must it, in common with other approaches, satisfactorily explain the variability of human behaviour, but must show how such variability can be explained within its central premise, the theory of inclusive fitness.

One approach to this problem, and the one which plays a major role in the work of P.L.van den Berghe (1979) on human families, is to emphasise not the variability of human behaviour, but its underlying similarity. Thus, on the theme of sex-roles he writes:

> Notwithstanding Margaret Mead's (1935, 1949) unconvincing attempt to support the thesis that all or nearly all differences in temperament and behavior between males and females are the product of arbitrary socialization, and that cultures vary randomly in the roles they assign to each sex, the ethnographic data clearly show the opposite (D'Andrade 1966; Stephens 1963;van den Berghe 1973). There is some variability in the details of what is considered feminine and masculine in different cultures, but there are also fundamental consistencies (van den Berghe, 1979, p.63).

Similarly, any evidence which appears to contradict the conclusion that:

> Given the consistency in cultural norms accross a wide range of societies, it is far more plausible that the norms *reflect* biological differences than they should *cause* behavioral differences (ibid. p.64, *itlaics in original*).

is dismissed as the result of rare or exceptional circumstances which prevent the expression of the expected biological universals. Thus, for example, the apparent problem posed by variability in human sex objects is explained as follows:

> Like all other animals, we are selected to mate with "sex objects" with whom we are likely to produce offspring. If we are male, that means women past menarche and before menopause. If we are female, that means men, preferably young and vigorous ones, though an older one will do if he has valuable resources to offer. In the absence of any such sex objects as will increase our fitness, we may satisfy our urge through masturbation, homosexuality, pederasty, even bestiality, but these are *pis-aller;* they are poor substitutes for the "real thing" (ibid, p.42, *italics in original*).

It is not difficult to see that an approach such as this, which attempts to deny the importance of both variation and *maladaptive* variation, cannot continue for long without having to face serious empirical obstacles. The last quotation from van den Berghe clearly illustrates the difficulties which must be faced. First, and most obviously, there is no evidence that sexual deviants are merely satisfying a fundamentally "normal" heterosexual reproductive desire, in the absence of an appropriate object. Case studies of fetishists, for example, which are discussed in later chapters, show that many such individuals have ample opportunity for reproductive sex, but simply prefer to engage in the deviant paractice.

Secondly, although van den Berghe certainly has a point when he accuses relativistic anthropology of exaggerating variation (see for example Freeman 1983), the force of this criticism is lost entirely when it is noticed that he merely replaces the exaggeration of one feature, with the exaggeration of another. Thus:

> ...human systems of kinship and marriage conform to only a few basic types. Underlying a great deal of variety in detail, human societies share much of their basic structure of kinship and marriage (ibid. p.87).

Perhaps the greatest difficulty, however, with this approach, is the need to regard such variation as does exist as adaptive. Thus van den Berghe writes immediately after the above:

> The variations, while they attest to our versatility in adapting to a wide range of environmental conditions, are themselves not random but adaptive (Ibid).

Whilst I shall argue later that it is certainly correct to claim that variations are not random, the claim that cultural variations are adaptive is impossible to sustain. Indeed, it is precisely this insistence which leads, in sociobiological explanations, to the sort of exaggerations of cultural similarity which we saw above. To further illustrate this difficulty, I wish now to turn to probably the most sophisticated attempt to date to give a biological explanation of cultural variation, Lumsden & Wilson's theory of gene-culture co-evolution.

Lumsden & Wilson's approach (1981, 1983) can be seen, at least in part, as an attempt to reconcile evolutionary explanation with the difficulty posed by significant cultural variation. They begin their analysis with the explicit recognition that their approach is intended to go beyond traditional sociobiology and "take account of the free-ranging activities of the mind and of the diversity of cultures created by them"(1981, p.2). The principal concept they employ to assist them in this task is the "culturgen" which is defined as follows:

> a culturgen is a relatively homogeneous set of artifacts, behaviors or mentifacts (mental constructs having little or no direct correspondence with reality) that either share without exception one or more attribute states selected for their functional importance or at least share a consistently recurrent range of such attribute states within a given polythetic set (Lumsden & Wilson 1981, p.27).

The significance of this concept, is that in principle at least, it accords a greater independence to culture than can be found in previous approaches. Indeed, it is this concept which enables Lumsden & Wilson to characterize the "central problem" in sociobiology as "the relation between genetic and cultural evolution" (1983, p.49), thereby further attesting to the importance in their model of recognizing the relative autonomy of culture. Culturgens, however, are not arbitrary; more precisely *existing* culturgens are not arbitrary.

For Lumsden & Wilson, actors face choices between culturgens, and these choices are made according to predisposing genetic factors: the "epigenetic rules" of mental development. Because cultural choices are ultimately

regarded in this model as the product of biological predisposition, it is argued that culture is kept on "an elastic but unbreakable leash" (1983, p.60) in the following way: as new culturgens emerge - presumably as the result of random biological mutation - they either die out or spread depending upon how far they enhance the inclusive fitness of those with the predisposition to adopt them. In this way, culturgens ultimately owe their existence to the contribution they make to the inclusive fitness of those with predispositions to adopt them, and so are not treated in any way differently from any other phenotypic effect.

Lumsden & Wilson juxtapose this mechanism against two other models which they argue are extremely unlikely to evolve. The first is pure cultural transmission, of the sort typical of sociological models, in which any cultural artifact is as likely as any other to be adopted. The second is pure genetic transmission, in which all behaviour is rigidly programmed genetically, and in which no independent existence at all is accorded to culture. Their reasoning behind their avowed rejection of these models appears to be sound. Actors predisposed to make behavioural decisions which enhance their inclusive fitness will have selective advantages over those who do not, and so it would be difficult to imagine circumstances under which individuals could evolve to make either adaptively neutral or maladaptive choices,which presumably would happen if there were no predisposing factors involved. Similarly, actors who approached all situations with a rigid biological predisposition to act in certain ways, would be in danger of being far too inflexible to cope with the unpredictable and novel in physical and social affairs. Further consideration, however, of the mechanism by which coevolution takes

place, reveals that the model is not as successful in accounting for cultural variation as its authors would claim.

Lumsden & Wilson claim that their model occupies a central position between rigid envirionmental determinism, and rigid biological determinism. To illustrate how this process is said to occur - what they term the "leash principle" (1981, p.13) - it is worth quoting their example of the evolution of the avoidance of brother-sister incest:

> The epigenetic rules that turn the mind away from incest lead also to cultural patterns which reinforce the rules - taboos and frightenng mythological stories. The coevolutionary process is set in motion: persons who conform to the aversion and the taboos leave more healthy offspring; the genes underwriting the avoidance of incest remain at a high level in the population; and the predisposition is sustained as one of the epigenetic rules. The two inheritence systems have evolved in concert - the genes that create the epigenetic rules and the culture which offers the choices on which the rules act. All of the events are linked in a circuit of causation, running from the genes to the rules of mental development to culture, and back to the genes again (1983, p.119).

The length of the biological leash which is said to restrain culture can be seen in the authors' concept of the "thousand year rule", which is regarded as the time necessary for "substantial genetic evolution.. [to].. occur in the epigenetic rules of cultural transmission, resulting in such effects as the genetic assimilation of culturgen preference and the assimilation of bias towards specific decision heuristics" (1981, p.295). In other words, Lumsden & Wilson argue that the genes which prescribe

culturgen choice can change radically in as short a time as one thousand years, or fifty generations.

Unfortunately this approach to culture is far indeed from satisfactory. Although Lumsden & Wilson clearly intend to accord to culture some independence from biology, in fact this is not achieved. Ultimately, culturgens are explained only in terms of epigenetic rules which promote their adoption, and because these epigenetic rules owe their existence to the contribution they make to inclusive fitness, *culture* is thereby explained only in terms of its contribution to inclusive fitness. In this respect, then, the coevolutionary theory does not offer any improvement upon the "cultural adaptationsism" which we observed in the work of van den Berghe. This leads again to the necessary adoption of the general strategy of systematically understating the significance of maladaptive cultural variations.

Furthermore, because the coevolutionary theory is constrained by time - in the form of the thousand year rule - it becomes impossible to recognise as significant the rapid emergence of novelty, with the effect that behavioural changes not ascribable to *biological* change tend also to be understated. This is illustrated by the following example:

> The innate tendency for human beings to learn one thing as opposed to another, in other words gene-culture transmission, is perhaps most dramatically illustrated by the phobias. These are the extreme fears into which people are plunged - stricken by nausea, cold sweat and other reactions of the autonomic nervous system. Phobias typically emerge full-blown after only a single unpleasant experience, and

> they are exceptionally difficult to eradicate, even when the victim is carefully reassured and coached by a psychiatrist. It is remarkable that the phobias are most easily evoked by many of the greatest dangers of mankind's ancient environment, including closed spaces, heights thunderstorms, running water, snakes and spiders. Of equal significance, phobias are rarely evoked by the greatest dangers of modern technological society, including guns, knives, automobiles, explosives and electric sockets, Nothing could better illustrate the peculiar and occasionally obsolete rules by which the human mind is assembled, or the slowness of man to adapt to the dangers created by his own technological triumphs (1983, p.70).

In fact, nothing better illustrates the drawbacks of maintaining a direct functional link between biology and culture. Statements such as "phobias are rarely evoked by the greatest dangers of modern technological society" are not just meaningless, they are simply incorrect. Even a cursory glance at case-studies of modern phobias, from Freud to the present day, reveals that almost any modern phenomena can become the object of a phobic reaction, and most certainly does not have to have been one of "the greatest dangers of mankind's ancient environment". Moreover, this functional analysis massively distorts the actual experience of phobic individuals, many of whom are obliged to go to ludicrous lengths to avoid contact with the phenomenon in question. Certainly Lumsden & Wilson's characterization of the functions of phobia : "better to crawl away from a cliff, nauseated with fear, than to casually walk its edge" (1981, p.85) bears little relationship to the reality of phobia, which not infrequently can

reach such heights of obsession that the most basic and routine of tasks is rendered impossible for fear of contact with the object of phobia. [2]

The difficulties to which the "leash priciple" inevitably leads can best be seen by considering its implications for the explanation of rapid social change. In particular the difficulty centres around the issue of how it becomes necessary to define similarity and difference. Ultimately, as we have seen, culture is reduced in the coevolutionary theory to biology; cultural forms can only become dominant if the majority of individuals in the population possess epigenetic rules favouring their adoption, and this can only come about by the ordinary processes of biological evolution. That is, a dominant cultural form must either be explained by the statistically impossible process of mass parallel mutation, or by the gradual effects of the reproductive advantage conferred on descendants of the bearer of an original mutation. Since the former can be discounted, we are left with the latter as an explanation of the process of cultural change. The flaw is obvious: such a process cannot account for significant cultural change between single generations, or indeed *within* generations. The consequence of this is that if the theory were to be retained, we would be left with no choice but to regard such apparent cultural changes as cultural *continuities*, and thereby deny them any significance whatsoever.

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[2] The more debilitating and maladaptive recognised phobias include "kinesophobia", defined as "the morbid fear of making any kind of movement for fear of self-injury, even where no danger exists", and "barophobia" defined as " morbid concern over the force of gravity, manifested by a sensation of weightlessness and a fear that one will fall into space". (from Chambers's Dicitionary of Psychiatry, 1967)

To illustrate this problem we might consider the effects of treating - as the above definition suggests we should - as two alternative culturgens, the phenomena of anti-Semitism, and racial toleration in twentieth century Germany. Now, whilst it is of course true that neither all Germans in the period 1918 - 1945 were anti-Semitic, nor all Germans in the period after 1945 have practised racial toleration, most scholars of this period would wish to recognize that there had been a significant shift in the proportion of German citizens taking each position. From the point of view of the Lumsden - Wilson coevolutionary theory, however, there would appear to be no way of recognising this apparently *cultural* shift, without making some extraordinary assumptions. It would be necessary to argue, for instance, that the intergenerational shift in opinion was brought about by the absence of epigenetic rules promoting anti-Semitism in the second generation, and that this was occasioned by the massive under-reproduction of anti-Semitic Germans active in the 1930s. Moreover, the rise of Nazism in the 1930s would itself need to be explained by the *over-*reproduction of an earlier generation of individuals possessing the epigenetic rules which predisposed them to manufacture or adopt anti-Semitism.

The consequence of not making these somewhat far-fetched assumptions, but still retaining the theory, is that that shift in question could not be recognised as significant, and therefore could not be accorded the status of a phenomenon to be explained. In other words, we would have to deny these phenomena - *contrary to the definition offered by the authors* - the status of independent culturgens. Of course it is, in terms of the theory, logically possible to do this, but such an admission would seriously affect

the explanatory power of the theory. Indeed, it would not be an exaggeration to say that by defining away problems in this manner, the theory is incapable of offering any explanation of social change whatsoever.

Similarly, the theory faces serious difficulties when called upon to explain behavioural changes which occur during the lifetime of an individual. For example, a sudden religious conversion in middle age, after a youth of strident atheism, would seem to qualify as a shift in culturgen choice, and one which might well have serious implications for the inclusive fitness of the individual in question. In terms of the theory it would be necessary to assume that co-existent in the same individual were epigenetic rules promoting competing and mutually exclusive culturgen choices. Again, this may be logically admissable, but such an argument would be in danger of denuding the concepts involved of any real meaning. This is because, ultimately culturgen choice can only be considered to be evolutionarily significant because it entails the selection of adaptively superior culturgens over adaptively *inferior* culturgens. It would only be possible to accept the coexistence of two alternative culturgens in the same individual if it was also argued that they possessed identical adaptive values. Doing this however, would lead to exactly the same difficulty as that advanced above; namely that the differences between culturgens - which we wish to recognize as empirically significant - could not, in terms of the theory, be so recognized, and so the event in question could not be explained.

Indeed, exactly as with ven den Berghe's approach, the co-evolutionary theory cannot ultimately recognise cultural variations which are not

directly attributable to biological evolution, and so must ultimately fail as a serious challenge to the prevailing orthodoxy of the social sciences. It is precisely this failing which led to the most damaging critiques, by conventional social scientists, of the entire sociobiological enterprise, and the consequent reassertion of the central dogma of traditional social theory.

The Cultural Critique of Sociobiology

One of the leading critics of attempts by sociobiologists to offer biological explanations of human culture was Marshall Sahlins (1977). Although Sahlins' critique emerged before the publication of the works discussed in this chapter - indeed in many ways they constitute a response to Sahlins - his work still represents one of the most forthright and unambiguous statements of the conventional social scientific response to sociobiology, and as I have suggested, co-evolutionary theory does not significantly depart from the fundamental assumptions of this discipline. Before moving on, in the next chapter, to examine the assumptions underlying the conventional position on the relationship between biology and culture, I wish to conclude the present discussion with a brief outline of Sahlins' argument against the biological analysis of culture.

The starting point of Sahlins' critique is the failure, discussed above, of sociobiology to account for the empirical realities of human societies. Thus he writes of his central example, kinship systems:

> ...no system of human kinship relations is organized in accord with the genetic coefficients

> of relationship as known to sociobiologists (1977,p.57).

Yet it is important to realise that his critique goes beyond the empirical failings of sociobiology; it is based equally upon the *a priori* conceptualisation of culture as a fundamentally irreducible determinant of human action. At the heart of Sahlins' analysis is the assumption that

> The reason why human social behavior is not organized by the individual maximization of genetic interest is that human beings are not socially defined by their organic qualities but in terms of symbolic attributes (ibid. p.61).

These "symbolic attributes" - or culture - are, for Sahlins, the ultimate determinants of all human reality, even biology:

> ..the objectivity of objects is itself a social determination, generated by the assignment of a symbolic significance to certain "real"difference even as others are ignored..(ibid, p.62)

And

> ...the biological givens such as human mating and other facts of life, come into play as instruments of the cultural project, not as its imperatives (ibid, p.63).

In this way, Sahlins effectively replaces the ultimate reduction of culture to biology, with its reverse: the ultimate reduction of biology to culture. Thus, whereas the conceptual assumptions of sociobiologists make it impossible to recognise the normality of cultural variation and

maladaptive behaviour, Sahlins appears to be endorsing a point of view whose assumptions make it impossible to recognise the normality of cultural *universals*, and *adaptive* behaviour. This is because for Sahlins, culture - as a final determinant of all reality, including human action - must be ultimately arbitrary, and so any cultural form which does correspond to the predictions of inclusive fitness theory, does so by pure chance.

Considered in this way, we can see that Sahlins' critique of human sociobiology, whilst partially grounded in a justifiable objection to its empirical inadequacies, proceeds as it does by virtue of its underlying assumptions about the nature of human culture. In short, the alternative explanation of human behaviour which Sahlins proposes depends utterly upon his contention that culture can legitimately be regarded as the ultimate and irreducible determinant of human action. It is this claim - fundamental, I believe, to conventional social theory - that I wish to examine in the following chapter.

CHAPTER THREE

Biology and Social Theory

Social theory is characterized by diversity. Yet there is one point on which all traditions appear to be united: the necessity of offering explanations at a level independent of biology. This fact of the irrelevance of biology is routinely stated in the first few pages of most introductory texts in sociology, and in more advanced statements of theory is generally taken to be so self-evident that to explicitly state it would be to insult the intelligence of the reader. My aim in this chapter, however, is to examine the precise nature and implications of this premise. I propose to do this by, first of all, considering the exact role which biology has conventionally been allowed to play in social theory. I do not intend to offer an exhaustive account of the formulations of each major theorist; such an exercise would be rendered redundant precisely becuase the role of biology is an issue on which social scientists are fundamentally agreed. Instead I wish to examine the role which biology *necessarily* plays in the conception of social theory as a distinct enterprise. Secondly, I wish to consider the adequacy of this typical formulation in the light of the earlier discussion of modern evolutionary theory.

The Basis of Social Theory

There seems little doubt that a fundamental rationale for social theory lies in the apparently self-evident inadequacy of biological theory in accounting for great behavioural diversity in the human species. That which is universal in human behaviour is all that can reasonably be

attributed to biology, and so everything about it which is interesting to social scientists - that is, its diversity - must be explained according to other factors. In this way, the conventional social scientific approach begins from a specifically anti-biological stance. It is important, however, to consider exactly what role biology *does* play in such explanations.

At the most basic level, the mere recognition that human behaviour is distinctly human, involves the implicit recognition that it is defined by a biological characteristic. Yet, ironically, it is exactly this biological characteristic - humanness - which is held to justify the isolation of human behaviour from biological explanation. The chief characteristic of this biological humanness, which is thought to justify the separation, is the human capacity for symbolic communication. As Sahlins has put it:

> In the symbolic event, a radical discontinuity is introduced between culture and nature. The isomorphism between the two required by the sociobiological thesis does not exist. The symbolic system of culture is not just an expression of human nature, but has a form and dynamic consistent with its properties as meaningful, which make it rather an intervention in nature (1977, p.13).

Thus, the capacity to communicate symbolically, which is biological, merely permits the operation of a higher level process, which is not referrable to the biological level. It is this assumption which permits the *sui generis* analysis of the social; that is, as constituted by its own processes and possessing its own dynamic. This relationship has been explicitly

articulated by Talcott Parsons, one of the few major theorists to address the issue:

> ...there can be no one-to-one correspondence between the properties of an organism and the personality's internalized contents of normative culture, and social role expectations (1977 p.172).

This analysis also presupposes - as well as the symbolic faculties - the biological capacity to internalize normative culture. Again, however, the assumption of this capacity, which in Parsons' later work is couched in terms of the Freudian super-ego (a claim to which I shall return), does no more than permit analysis to proceed at a higher level, and in a way entirely unconnected with, the capacity in question. In other words, once it is accepted that human biological apparatus exists which enables members of the species to use symbolic communication (including symbolic thought), and to internalize normative orientations, analytical focus falls exclusively upon the particular symbols and normative orientations in question.

It is perhaps something of a simplification, but I think a justifiable one, to say that debate amongst social theorists has been almost entirely about the relative importance of, and the differing contents of, symbols and the internalization of normative orientations. Thus, for instance, Marxist analysis differs from the structural-functionalism of Talcott Parsons, in that for the former the content of symbols which are internalized as normative orientations are determined by economically constituted social relations, whilst for the latter by the equilibrating processes of the social system and its various sub-systems. Although the differences between the

two are obviously fundamental, they have in common the assumption of these basic biological capacities which permit *sui generis* analysis at non-biological levels. Similarly, "structural" social theories such as these differ from phenomenological sociology, including symbolic interactionism, in the degree of social structural determination accorded to the content of symbols, their integration into larger coherent systems, and thus the importance of direct internalization. Taken for granted, however, is again the purely facilitating nature of these underlying biological capacities.

Social theory has thus generally been conceived as having a relationship with biology analogous to the relationship between biology and physics or chemistry. Thus Sahlins writes of the example of the relationship between gravity as a physical law and the biological process of natural selection:

> ...gravity constitutes a limit to biological forms: every stage in the life history of every species has to conform to it, and any mutation that might seek structurally to do otherwise does so at its peril. But a limit is only a negative determination; it does not positively specify how the constraint is realized. Within the limits of gravity, every stage of every species has developed; hence such limits explain nothing of the *differentia specifica* of life forms, but only the failure of any of them to exceed certain tolerances. Going still farther, it is possible to say that physical or chemical properties, such as gravity, are means employed by the biological forms in the production of the organism (1977,p.64, *italics in original*).

In exactly the same way it is argued that biological "laws" - such as those of evolutionary biology - can explain nothing of the "differentia specifica" of

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human behaviour, merely that such laws provide basic conditions or, if we wish to go farther, "means" for the organization of such behaviour at higher levels. Thus whilst Sahlins is quite prepared to grant that "cultural life must conform to natural laws" (p.65), this is immediately qualified as follows:

> But a law of nature stands to a fact of culture only as a limit does to a form, a constant to a difference, and a matrix to a practice. It will never be possible to explain the cultural properties of any such fact by referring to the underlying contents of a different order (p.66).

He then concludes:

> How then does biology figure in culture? In the least interesting ways as a set of natural limits on human functioning (p.66).

Sahlins thus captures the essence of the social scientific position on human biology. Of course social theory must recognize the existence of biological constraints - principally the possession of symbolic and internalizing faculties - but this should impinge no more on the nature of the explanation sought than the recognition that social life is constrained by the laws of gravity. Once the social theorist has recognized the facts that human beings can use symbolic language, internalize normative culture and do not fly around randomly in space, the theoretical enterprise can proceed untroubled any further by the concerns of evolutionary biologists and physicists.

But what precisely is the nature of the constraint which biology is thought by social theorists to impose upon higher orders of explanation, and what exactly can this constraint tell us about the specifics of any particular case? The social theorist who has perhaps dealt most fully with this question is Claude Levi-Strauss. Because Levi-Strauss' views on the matter are clearly articulated, and because his view travels as far down the biological road as the assumptions of conventional social theory will allow, it is worth spending a little time on his argument, as it nicely illustrates the radical distinction which social theory inevitably makes between nature and culture.

Levi-Strauss begins his answer to this question with the familiar assertion that what distinguishes humans from animals, and hence the study of human societies from animal societies, is the human (ultimately biological) capacity for symbolic communication. Following de Saussure's fundamental distiction between the signifier and signified (1983), and his assertion that the relationship between the two is ultimately arbitrary, Levi-Strauss develops a theory of culture in which the elements are formally equivalent to the elements of a language (Levi-Strauss 1966). Thus, culture - defined by Levi-Strauss, as by others - as that which is particular, and hence not part of universal biology, (Levi-Strauss 1969, Ch.1), is conceived as a system of signification in which the signs bear an entirely arbitrary relationship to that which they signify. Like language, the elements of culture derive their meaning, for Levi-Strauss, not from any necessary relationship with that which they represent, but from their relationships with other signs as part of a total system of signification. This is the *synchronic* aspect of culture. Levi-Strauss argues, then, that the

actual signs which constitute cultural systems - such as totemism, kinship systems and myths - could have been other than they actually are, in exactly the same way as for a word; convention alone dictates that in a particular context one is obliged to refer to a pink animal with four legs and a twisted tail as a "pig", and not anything else.

In this respect, Levi-Strauss' argument does not differ essentially from the general social scientific position that the contents of culture need to be understood *sui generis*. Where he goes further than most, however, is in the degree of attention he pays to the underlying biological capacities to use and manipulate symbolic relationships. Whilst most social theorists are prepared to accept these as a basic condition of their analysis, Levi-Strauss makes it a fundamental purpose of his analysis to explore the extent to which the characteristics of the biological human brain determine the nature of culture. This very enterprise appears to be founded on a paradox: we have seen that for Levi-Strauss the content of cultural categories is, like de Saussure's conception of the linguistic signifier, arbitrary. How then can culture be both arbitrary and determined by the organic nature of the human brains which facilitate it?

The answer provided by Levi-Strauss is that since human brains everywhere share basic symbolic faculties - that is, the capacity to make and maintain connections between arbitrary signifiers (binary logic) - these underlying principles should be present in all products of human mental activity everywhere. Despite the variety in cultural contents, which are arbitrary - and in Levi-Strauss' view rooted ultimately in the materialist dialectic of the specific milieu - the underlying *structural* principles of any

particular cultural phenomenon must be the same. The important point to notice is that, as a reading of some of Levi-Strauss' actual analyses makes clear (eg Levi-Strauss 1966), the structural priciples in question are fundamentally algebraic transformations of structure. In this way, cultural contents remain arbitrary and essentially relative, whilst all cultures everywhere are said to display a common set of structural transformative principles.[1]

Whether or not Levi-Strauss is successful in demonstrating the common structural properties of diverse cultures has been a much debated issue (see Leach 1970), but the adequacy of his account in this respect is not at issue here. What is of great importance to the argument I am developing is that even an apparently extreme reductionist argument like that advanced by Levi- Strauss, which explains the structure of human culture in terms of the universal architecture of the human brain, retains the fundamental assumption of the arbitrary, that is, *sui generis*, nature of the *contents* of culture. In other words, the meaning of cultural symbols (that is, the signifieds to which they are arbitrarily connected) has nothing whatever to do with biology; they relate instead to an entirely different (non-biological) order.

The Social Basis of Motivation

I wish now to broaden the foregoing discussion of the non-biological basis of social scientific explanation, to consider the exact implications which

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[1] Particularly clear secondary accounts of the details of Levi-Strauss' argument may be found in, for example, Leach (1970), and Badcock (1975).

this has for the motivation of action. It is important at this stage to emphasise that by the "motivation of action" I mean the *directed* use of physical or mental energy. My reasons for employing this definition will become clear later in the chapter, but for the time being let me stress that this does not confine motivated action to actions which are the outcome of *intention*(although this will obviously be important). Rather, I mean to emphasise merely that motivated action involves the expenditure of energy in some specific ways as opposed to others. In this way, I distinguish motivated actions only from the logical category of random actions, which, by definition, involve the expenditure of energy in ways which have no explanation.

In terms of this definition, it can be seen that conventional social theory necessarily gives an account of motivation in terms of non-biological orders. This is because the assumption of non-biological *sui generis* levels of analysis inevitably invloves the assumption of causal connections between these levels of analysis and the specific doings of individual actors. Put more basically, if it is legitimate to analyse social behaviour as forming part of a larger independent system (broadly conceived), which has its own properties and processes, and is distinct from other such systems, then because this system only exists by dint of specific actions being performed by specific individuals, it is necessary to assume that such actions are causally determined *by that system.* This recognition is made explicit, again in the works of Talcott Parsons:

> The prerequisite of adequate motivation gives us one of the primary starting points for building up the concepts of role and of institutionalization. Fundamentally the

> problem is, will the personalities developed within a social system, at whatever stage in the life cycle, "spontaneously" act in such ways as to fulfill the functional prerequisites of the social systems of which they are parts, or is it necessary to look for mechanisms, that is, modes of organization of the motivational systems of personalities, which can be understood in direct relation to the socially structured level or role behavior? The older "psychological" view that societies are resultants of the independently determined "traits" of individuals would take the first alternative. The modern sociological view tends to emphasize the second (Parsons, 1951 p.31).

Although perhaps less explicit, no less of an assumption of the non-biological origins of motivation exists in the self-conscious disavowal of the structural and systemic properties of social action found amongst symbolic interactionists (see, for example, Blumer 1969). Here action is thought less to be determined by the involvement of the actor in formal concrete *sui generis* systems, but more by the self-reflective manipulation and interpretation of meaningful symbols. But precisely because such symbols are only meaningful because they belong to a non-biologically constituted system of arbitrary signification, which therefore has an existence of its own, motivation must once again be considered to determined by non-biological demands.

In effect, the social scientific requirement of treating motivation as a means by which the processes and dynamics of normative and symbolic systems are played out, denudes the concept of motivation of any real significance. One indication that this fact is beginning to be explicitly realized by social theorists can be found in Anthony Giddens' discussion

of motivation in his major theoretical work, The Constitution of Society (1984). In this he writes:

> I distinguish the reflexive monitoring and rationalization of action from its motivation. If reasons refer to the grounds of action, motives refer to the wants which prompt it. However, motivation is not as directly bound up with the continuity of action as are its reflexive monitoring or rationalization. Motivation refers to potential for action rather than the mode in which it is chronically carried on by the agent. Motives tend to have a direct purchase on action only in relatively unusual circumstances, situations which in some way break with the routine (p.6).

In this passage Giddens explicitly replaces a concern with the motivation of action with its routinization. Effectively, in terms of my definition, he is saying that actions are motivated principally by habit. It is true that he he says that such habit is an essential requirement of the (presumably biologically based) personality, but crucially, this is qualified by the claim that its specific *content* is integral to the institutions of society:

> Routine is integral both to the continuity of the personality of the agent, as he or she moves along the paths of daily activities, and to the institutions of society, which are such only through their continued reproduction (1984 p.60).

In this way, all that can be said to be biologically motivated is the formation of habits. To understand the content of such habits however, we need to understand the exact constitution of the social institutions

whose "reproduction" the habits in question serve. [2] Motivation, therefore, insofar as it refers to the specific direction in which energy is expended - that is, to the content of actions, and not merely to action itself - is in Giddens' formulation a function of the specific institutional milieu of the actor.

We may summarize the implications of sociological analysis for the motivation of action as follows: biology supplies the general conditions for motivated action, whilst the particular social milieu (however it is conceived by the various formulations) supplies the specific contents of any actual instance of motivated action.

Motivation and the "Cultural" Biologists

It should not be thought, however, that this fundamental separation of the capacity for motivated action, and the nature of specific motivations is restricted to conventional sociological analyses. On the contrary, similar assumptions, albeit cloaked in deceptively Darwinian language, have been made by a number of seemingly orthodox modern evolutionary biologists, evidently dissatisfied with the empirical shortcomings of the sociobiological analysis of culture (eg Cavalli-Sforza and Feldman 1981, Cloak 1975, Boyd & Richerson 1985, Dawkins 1989). For the purposes of illustration, and before moving on to an analysis of this understanding of

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[2] This assumption of the basic need for habit formation, irrespective of the contents of such habits, is nicely illustrated by Giddens' use of Bruno Bettelheim's account of life in a prisoner of war camp in which the tendency of older prisoners to ape their captors is seen as an evidence of such habit formation.(Giddens 1984,pp60-64).

motivation, I wish to offer a brief discussion of one such model, namely Richard Dawkins' concept of the "meme" (Dawkins 1989, Ch,11).

Dawkins begins his introduction of the concept of the "meme" with the standard social scientific claim that humans are different from animals because they possess "culture". He then asserts that "cultural transmission is analogous to genetic transmission in that, although basically conservative, it can give rise to a form of evolution" (1989 p.189). What is crucial about this is that cultural transmission is seen as "analogous" to biological evolution. His principal justification for this assertion is that units of culture share with genes the basic property which permits evolution to occur, that is, they are *replicators*. These units of culture are what he terms "memes", and like genes, although crucially, independently from the influence of genes, they either spread or die out depending on the degree of success they display in making replicas of themselves. As an example, consider the following:

> Consider the idea of God. We do not know how it arose in the meme pool. Probably it originated many times by independent 'mutation'. In any case, it is very old indeed. How does it replicate itself? By the spoken and written word, aided by great music and great art. Why does it have such high survival value? Remember that 'survival value' here does not mean value for a gene in a gene pool, but value for a meme in a meme pool. The survival value of the god meme in the meme pool results from its great psychological appeal. (1989, p.193).

Dawkins, and this is perhaps where his analysis is most surprising, is quite explicit that he does not mean to reduce "psychological appeal" to

"biological advantage", hence ultimately explaining the presence of a meme by its genetically adaptive significance (as was the case with Lumsden & Wilson's "culturgen" examined in the last chapter). In fact the very idea of an *analogous* process of mutation in the meme pool precludes this possibility because, by definition, mutation is random and so must have an equal probability of emerging in any human brain. Equally, because the spread of mutant memes is held to be determined only by the qualities of the meme itself (exactly as with genes) it would be impossible to argue simultaneously that some human brains are more likely than others to adopt a particular meme. The following example illustrates graphically that the contents of Dawkins' memes are quite independent of genetic influence:

> A gene for celibacy is doomed to failure in the gene pool, except under very special circumstances such as we find in the social insects. But still, a *meme* for celibacy can be successful in the meme pool (1989, p.198).

By advancing this process of "meme selection" Dawkins is doing nothing less than removing the content of human actions from the sphere of biology, and thus, in terms of motivation, is advancing an argument identical to that of conventional social theory. Of course his insistence that memes follow patterns of development formally equivalent to genetic natural selection, would probably be regarded as rather simplistic and naïve by most social theorists, as would his contention that unitary "pieces" of culture can be isolated. Neverthelss, by arguing for a strictly analogous relationship between the processes of genetic and cultural evolution, Dawkins is quite explicitly endorsing the *sui generis* analysis of human social behaviour, and its consequent isolation from biological

motivation. Ironically, the most radical of gene-selctionists, turns out to be a sociological determinist, *par excellence.*

Evolution and the Motivation of Action

In what remains of this chapter, I wish to subject to scrutiny, the assumptions about the motivation of action which I have outlined so far. To recapitulate, the essence of the conventional social scientific position as I have represented it, is that whilst human beings of course possess fundamental biological capacities which facilitate motivated action, these capacities tell us nothing about the content of the actions which they are motivated to perform. Insofar as motivated action may be considered as the expenditure of energy in specific ways, we may say that, from this point of view, motivation must be considered an exclusively *social* or *cultural* phenomenon; that is, as determined by essentially non-biological factors. Biology is to the motivation of action as the engine of the motor car is to the journey: it is, to be sure, a necessary prerequisite, but a knowledge of its nature, no matter how detailed and accurate, could never tell us the car's destination.

A consideration, however, of the processes by which the facilitating biological capacities came into being, suggests that this convenient separation may not be as justified as its proponents would wish to believe. Action, as even the most hard-line social theorists willingly accept, depends upon physiology. In particular human action, as we are so frequently told, depends upon the unique architecture of the human brain, as it is this which permits the uniquely human activites of symbolic

communication and thought, and the internalization of normative culture. But what is the origin of this unique organ? It is unlikely that even our sociological hard-liners would wish to dissent from the view that it came about as the result of thousands of generations of natural selection.[3] To be precise, to the best of our knowledge, the human brain, with all its unique "capacities" came about in accordance with the evolutionary principles outlined in chapter one; namely, that the genes which prescribed its development spread and dominated the gene pool because they were relatively more successful than others in making copies of themselves. In other words, genes for human brains ultimately survived because they produced phenotypic effects in their bearers which enhanced their inclusive fitness.

The question to which we are now finally forced, is, to what extent can the attributes accorded to human brains by social scientists, be said to constitute a sufficient selective advantage to explain their evolution? Put more directly, and in the context of motivation, to what extent can the *capacity* to to be motivated, that is, to expend energy in specific, but *biologically arbitrary* ways, be said to constitute a sufficient selective advantage for the evolution of the human brain? My own view of the matter, and one which I believe follows inextricably from an understanding of the process of natural selection, is that such an assumption denies the very possibility of its evolution.

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[3]Although, amusingly enough, Anthony Giddens appears to think that Darwinism has had its day in the natural sciences, whilst still being of use to social scientists (Giddens 1984, p.231).

To see why this is so, it is necessary to return briefly to the argument outlined in chapter one. There I emphasised that the process of natural selection must be considered in teleological (or teleonomic) terms; that is, the *effects* which a gene has for its own reproduction, are causally related to its own existence in the species' genome. For example, the genes underwriting the human heart owe their existence to that fact that the phenotype they produced (ie, actual functioning hearts), was relatively advantageous (ultimately in terms of inclusive fitness) to those individuals who possessed it. What is essential about this example, however, is that it is the specific effects of the heart which are responsible for its existence in the genome. If, on the other hand, the heart was an organ merely *with the capacity* to perform in the way it does, but which did not actually do this, there would obviously be no selective advantage attached to it whatsoever, and so it could not evolve.

The point I am making here is that because evolution is an ultimately teleological process, that is, is fuelled by effects, mere potentialities cannot evolve. Returning to the case of human behaviour, it makes no sense to say, therefore, that human *capacities* for internalization, symbolic communication etc. evolved by natural selection. Rather it is necessary to say that the capacities in question evolved because they produced specific adaptively advantageous effects for the inclusive fitness of the individuals who possessed them. The problem which is therefore posed by the conventional social scientific conception of motivation is clear: if the biological nature of human capacities can tell us nothing about the specific contents of the actions which they facilitate - which are, biologically

speaking arbitrary - it is impossible to assume that the capacities in question came about by natural selection.

This is not to say that the directed (that is, motivated) expenditure of energy needs to be determined entirely from within the organism, however. One very obvious example where this is not the case, is photosynthesis in plants. Here the capacity to photosynthesise is all that is genetically determined; the action of performing photosynthesis, which has a determinate effect on whether the plant in question reproduces, is consequent upon the presence of an environmental factor, namely sunlight. What is crucial about this situation, and that which separates it from the case of human action, is that the environmental motivator (as it were) - sunlight - *pre-exists* the genetically determined capacity to photosynthesise. Thus, if there was no sunlight present when the first plants with the genetic mutations underwriting the capacity to photosynthesise emerged, then no photosynthesis would have taken place, and so the capacity would not have evolved.

What all of this means for the case of human action, is that for culture (broadly understood) to be the primary motivtor of action, as conventional social theory claims, it would have needed to have existed prior to emergence of the physiological apparatus which facilitates it. In other words, if the contents of human actions are determined by variously conceived *sui generis* social factors, then these factors would have had to be already in existence when the first biological capacities to be motivated by them emerged. Moreover, they would have needed to possess a content which not merely motivated action, but which motivated actions

whose contents had adaptively advantageous effects for the inclusive fitness of the actors in question. The difficulty with this is obvious: as these motivating factors cannot have an existence independently of human agency, then they cannot have existed prior to the emergence of the biological apparatus which facilitates human agency.

Even if this logical obstacle could be overcome - which it could not - there remains the evolutionary problem of explaining the stability of a motivationally neutral population. Essentially what this means is that such theories implicitly assume the presence of genetic predispositions for behavioural manipulation. As soon, however, as one mutant gene emerged in the population which conferred even a slightly advantageous motivational bias to its bearer, this gene would be the subject of enormous positive selective pressures.

Of course these objections apply just as strongly to Dawkins' (and the other "cultural biologists'") attempt to understand culture, as they do to conventional social theory. His suggestion that memes bear a strictly analogous relationship with genes leads to exactly the same radical separation of the capacity for motivation from the contents of motivated actions. Indeed, Dawkins' formulation is, astonishingly, even more explicit in this respect, in that he claims that "we do not even have to posit a genetic advantage in imitation" (Dawkins 1989, p.200), which is held to be the principal capacity involved in meme replication.

It is I think possible to see that in the light of our knowledge of the evolutionary process, the conventional reliance on motivational plasticity

simply will not do. To begin with the assumption that the actual content of motivation can be understood as wholly independent of the capacity to be motivated, is to implicitly deny the possibility of the involvement of biological apparatus in human action, and hence to allow the explanation of action to collapse into metaphysics. In reaching this conclusion, I must emphasise that I am doing no more than following Sahlins' advice that "cultural life must conform to natural laws" (1977, p.65; see p.44 above), but have simply traced the implications of "natural laws" for "cultural life" to their logical conclusion.

The Paradox

If the arguments I have advanced so far are accepted, it should now be clear that a consideration of the principles of evolutionary biology introduces a paradox into the explanation of human behaviour. Such behaviour cannot be explained in terms of its adaptive significance as the sociobiologists would wish, because, very simply, it is too diverse, prone to variation and apt to be injurious to the inclusive fitness of the individuals in whom it is displayed. At the same time, however, theories which, by their logical separation of the biological ability to act, from the contents of actions, manage to offer empirically compelling accounts of human behavioural diversity, cannot be sustained when the implications of evolutionary biology are fully considered.

The conclusion to which we are therefore drawn is that the only satisfactory account of human behaviour is one which is able to account for the contents of actions in evolutionary biological terms, whilst at the

same time, taking full recognition of the fact that human action is diverse, and perhaps more to the point, is often maladaptive. On the face of it, this enterprise sounds quite evidently contradictory: if an action is motivated in accordance with evolved mechanisms, such mechanisms could only have evolved by motivating actions which are relatively advantageous to the inclusive fitness of their bearers, and so cannot simultaneously be held to motivate those which are not. Further consideration, however, reveals that this situation is not as paradoxical as at first it appears. It is to the solution of this paradox that I now wish to turn.

CHAPTER FOUR

Evolution, Psychology and the Motivation of Action

To see that the paradox on which I concluded the last chapter is really nothing of the kind, it is necessary to briefly return to an argument outlined in chapter one. Here, it will be recalled, I emphasised that an important reason why the charge of "Panglossian optimism" is not an appropriate objection to evolutionary theory, is the phenomenon of "time lags" (pp16-17 above). Essentially, this refers to the fact that environments can change at a quicker rate than organisms can evolve, and so it is quite possible that an organism could display a genetically based trait which is maladaptive in the environment in which it is observed, but which was adaptive in the environment for which it evolved. In principle, this fact means that even "fatal" genes - that is, those which *prevent* their own reproduction - could be explained in terms of inclusive fitness theory if it could be shown that they produced an adaptive phenotype in some previous environment. Obviously such a situation would be evolutionarily unstable, and could not be expected to obtain for long without either the selection of appropriate mutations, or as would be most likely in such a case, extinction. More frequently, however, time lags can be expected to account for phenotypes which are not wholly "fatal", but which display insufficient adaptive function to account for their selection; the less diminution in adaptive function the changed environment produces, the longer the adaptation in question is likely to survive. What this fact compels us to remember, however, is that evolution is most certainly not a spent force, whose completed work we may now retrospectively examine. On the contrary, evolution continues

even, perhaps especially, as environments change, and will continue long after our current examinations of its consequences to date are complete.

Clearly, consideration of the effects of time lags in evolutionary explanations is an enormously complicating factor. What it first of all suggests is that it is inadequate to confine our examination of the evolutionary significance of any observed trait to its currently observed effects. Rather, we should be concerned with the consequences of this trait in the environment for which it evolved. The difficulty with this is that, depending upon the species and trait in question, data on this may be sparse and inaccurate, and rarely will have been subjected to the rigorous scrutiny which a complete analysis of evolutionary significance would demand.

A second implication of this, and one which complicates matters considerably, is that the environmental changes may affect the nature of the trait itself. This is because to speak of a "genetically determined" trait is in fact somewhat misleading, in that in reality genes and environments *interact* to produce phenotypes. The word "environment" is of course used here in a rather loose sense, to include both the external environment, and the internal environment of the organism, but it is clear that both are crucial to the ontogenetic development of a particular trait. A basic example of this effect would be plants which grow in inadequately lit places, and as a result grow long and spindly, and so have abnormally weak stems. The weakness of the stem is a trait which must be attributed to genetic determinants, which dictate how the plant will interact with various environments, and to the absence of the

environment which would produce an adaptive response. In other words, an environmental change has occurred which changes the nature of the phenotype, without affecting the genotype. Yet the explanation of the effect, which is maladaptive, must still be given in genetic and ultimately adaptive terms. This is because even though the response was maladaptive, it still took place because the plant possessed genetic determinants which caused a particular response to a particular environment, and the possession and nature of these genetic determinants are as they are because they were subject to the normal evolutionary process. The proof of this lies in the fact that had the genetic determinants been different, the environment in question would have interacted differently with the genetic material of the plant to produce a different response.

Sociobiology or Evolutionary Psychology?

Whilst all of this may be of limited significance for the case of plants, when the same reasoning is applied to the case of human behaviour it is clear that the implications are very great indeed. No-where do we find so perfect an example of the operation of time lags than in the evolution of human behaviour. Here is a situation in which the entirety of recorded history constitutes a period of evolutionary time - perhaps five hundred generations - in which none but the most superficial genetic evolutionary changes could occur. Contrasted with this, are the two million years or so of pre-historic human evolution, in which our primal ancestors lived a predominantly hunter-gatherer existence.

It follows from this and the foregoing argument, that it is therefore quite wrong to assume that behaviour observed in modern human beings should have currently adaptive consequences. What this means, then, is that the paradox on which I ended the last chapter need not be such when the crucial factor of the environment is brought into consideration. This is because since genetic adaptations only manifest their effects in certain environments - specifically those for which they evolved - the absence of the environment in question is likely to have a detrimental effect upon the manifestation of the adaptive phenotype. At the same time, however, this recognition allows us, in principle, to pursue an analysis of the maladaptive trait along adaptive genetic lines. What this means for human action is that the motivation of maladaptive or novel actions which, as we have seen, could not be explained by sociobiological approaches, can still be accounted for in evolutionary terms, and without recourse to the logically and empirically unacceptable assumptions of conventional social theory.

To see precisely how this principle offers a solution to the paradox, it is helpful to make a distiction between the approach of "sociobiology", as already discussed, and "evolutionary psychology". Fundamental to this distinction is the way in which each approach conceptualizes exactly what it is that evolves. For the first it is behaviour; that is, the actual concrete actions of the organism in question. For the second, the view which I am advocating here, it is psychological mechanisms. At first sight this distinction seems rather dubious; after all, have I not been insisting from the beginning that evolution is fuelled by consequences, that is, the actual behaviour of the organism in question? This is perfectly true, but further

consideration reveals that to say that actual behaviour determines the evolution of a trait is, in fact, quite different from saying that behaviour *is* the trait which evolves. As the arguments advanced so far in this chapter make clear, what evolves are genes which, *under certain environmental conditions*, produce phenotypes which have beneficial effects for the inclusive fitness of the bearers of these genes. In behavioural terms, what evolves are genes which, *under certain environmental conditions*, produce behaviour which is adaptive. The fundamental point therefore is that there is nothing about the genes which *guarantees* that the adaptive behaviour will develop, since the genes merely produce the physiological apparatus which under the right conditions produce this behaviour.

It is important to emphasise that this formulation is fundamentally different from the social scientific position outlined in the last chapter. What I am definitely not saying is that the environment motivates action. As I have already argued, were this the case then the apparatus in question could not have evolved. What I am saying is that, in accordance with the fundamental principles of natural selection, the motivation of action cannot be treated as separate from the apparatus which permits it, and so the nature of both must have evolved in accordance these principles. Since motivated action takes place in an environment, however, and more exactly in response to environmental events, dramatic changes in the nature of these events will undoubtedly have implications for the actions which are thereby motivated. To be specific, such a formulation assumes that there is a finite repertoire of possible responses to the environment, which were adaptively appropriate in the environment of evolutionary adaptedness, but which in the absence of the evolution of

new responses, continue to be called forth by features of the new environment.

Quite clearly, since the environmental features which elicit motivated actions are different from those for which the motivations in question evolved, the consequences will not necessarily be adaptive. But equally important is the fact that motivational responses to novel environments are not random. This is because to assume that a given action has an equal chance of being motivated in response to any enviromnent, is to fall into exactly the same trap as conventional social theory, namely, to divorce the content of an action from the evolution of its facilitating biological apparatus, and thus implicitly deny the possibility of the evolution of that apparatus. On the contrary, motivated actions which are elicited by novel evironments can only be assumed to be so elicited because of some *similarity* between the novel environmental feature and the evolutionary environmental feature in response to which the motivation evolved.

It is the nature of this essential similarity between the current environment and the environment of evolutionary adaptedness which, I suggest, justifies analysis at the level of psychological mechanisms. To appreciate why this is so, it is necessary to consider precisely what it means to say that the two environments bear a similarity with each other. Again, this may seem a rather dubious point, particularly since so far I have argued that maladaptive behaviour need not present a problem for evolutionary explanations when environmental *differences* are considered. The point I am making, however, is not that the two

environments are *objectively* similar, in the sense of eliciting motivations which would prove adaptive in either case, but rather they are similar from the point of view of the motivational system of the individual concerned. In other words, the motivational system, or more simply, the *mind* of the individual, responds to the novel environment *as though* it was responding to the environment of evolutionary adaptedness. This is for the very simple reason that it has not yet evolved responses to any other environment and so has no option, as it were, but to treat features of the modern environment as though they are the features of the evolutionary environment which they most resemble.

The point I am attempting to make can be seen best by considering the analogy of blindfolding a competent typist, arbitrarily altering the order of the typewriter keys, and then asking the typist to type a well-known passage. Obviously, the result will be the production of a random series of letters. The motivation, however, that is, the expenditure of energy in particular ways as opposed to others (pressing certain keys in a certain order) remains the same, and although in the changed environment of randomly altered keys, the observed outcome is random nonsense, the motivated action (the keys which are actually pressed) is far from random, and indeed quite explicable in terms of the original environment. To pursue this analogy a little further, and in illustration of my point about the similarity of environments, we may say that the new environment of altered keys, is similar to the old environment of the standard keyboard, in that whilst they are of course crucially different in terms of effects, the closest thing to the new keyboard in the original environment of the typist is the old, standard keyboard, and so the typist, lacking any "adaptations"

for the new environment (i.e. knowledge that the order of the keys is different), behaves as though they are in the old environment, and presses the keys that *would* have led to the desired effect.

What this anaolgy illustrates is that the crucial determinant of the similarity between the two environments is the nature of the motivational system of the actor. In the most abstract sense, environmental similarity is determined by the interpretative processes of the actor; to return to our analogy, the typist *interpreted* two different environments as the same, and on that basis performed the actions they did. However, what must be remembered is that the interpretative processes of the actor are part and parcel of the evolved mental apparatus, and so must operate in an evolutionarily directed fashion. In other words, the capacity for interpretation can only have evolved to be selective, and so novel environments must elicit the responses they do because of certain evolved (and thus, ultimately adaptive) innate rules or principles of interpretation.

It may of course be objected that this application of the concept of *interpretation* fails to recognize the uniqueness of this specifically human activity. But to make this objection, is to once again fall into the trap of failing to recognize that the biological apparatus which permits this unique ability evolved by natural selection. Insofar as all behavioural adaptations involve biased responses to particular environments - which of course they must to be adaptations at all - it is permissable to say, in an abstract sense, that *all* organisms interpret their environments. This is certainly not to deny that different species evolve quite different means by

which this is achieved, and that the human method is probably by far the most complex. But insofar as evolutionary theory forces us to recognize that all behaviour is evolutionarily biased behaviour, and that all behaviour is environmentally specific, it is essential to realize that the admittedly great differences in species-specific mechanisms, are fundamentally quantitative rather than qualitative.

What all of this amounts to, is that human resposes to environments (like those of all other species) must be considered *meaningful,* that is, carried out because the environment has some *significance* for the actor. But unlike the usual sociological formulation of this, both the content of the meanings which actions have, and the processes by which certain environments are given certain meanings are evolved biological givens, and not arbitrarily imposed cultural imperatives. In other words, action must be seen as the motivated signifier of a biological signified, rather than the arbitrary signifier of a cultural signified. This suggests not merely that the evolved human mind should become the centre of focus - evolutionary psychology - but it suggests that the mind we are looking for, and the world it creates, are of a very specific kind.

Evolutionary Psychology: *Reductio ad Absurdum?*

Before turning to a consideration of precisely what kind this is, I wish to say something about the objections which are commonly raised against the kind of reductive analysis which I am advocating, and in the process, help to clarify further exactly why I propose to take this as my level of analysis. It is often argued, as part of the sociological concern with *sui*

generis analysis at the level of the social, that to reduce explanations to the level of individual psychology is to do precisely the opposite of that which I am advocating, and fail entirely to account for the meaningful nature of human conduct. One version of this, and one which is essentially a philosophical rendering of the central assumptions of conventional social theory, is Marjorie Grene's critique of what she takes to be the sociobiological conception of mind (Grene, 1978). Here Grene defines the mind by means of a quotation from A.J.P.Kenny:

> "To have a mind" Kenny writes, "is to have the capacity to acquire the ability to operate with symbols, in such a way that it is one's own activity that makes them symbols and confers meaning upon them" [Kenny and others, 1973, p.47] (Grene 1978, p.214).

Grene then goes on to clarify the terms of this:

> The word *symbols* here refers to artifactual entities, assigned by convention to assume certain roles in the social practices of their users; and the word activity here means responsible activity, characteristic of the kind of center of action that can do right or wrong, be praised or blamed (ibid. italics in original).

It is very clear from this, that the mind is understood in this analysis as exactly the same in all crucial respects to that assumed by conventional social theorists. That is, the mind is the "capacity to acquire the ability to operate with symbols", whilst the symbols themselves are entirely "artifactual" and "assigned by convention to assume certain roles". In other words, the contents of the mind, understood as the fundamental

meaning or significance of its products, are extrinsic to the biological nature of its capacities, and are determined by arbitrary social convention. It is this assumption which, Grene argues, demands the essentially non-biological analysis of mind. She thus concludes her paper, following a passage from Sahlins:

> ...it is, again, the capacity to acquire the ability to enter into just such symbol-constituting and symbol-constituted activities that is definitive of mind and so renders mind, like culture, irreducible to its biological and in particular to its genetic conditions (Grene, 1978, p.224).

It is important at this point to consider precisely what it means to say that mind cannot be reduced to "its biological and in particular to its genetic conditions". Evidently, Grene is here failing to distinguish between ultimate and proximate causation as they figure in evolutionary explanations (see ch.1 above). To briefly repeat the distinction, ultimate explanations refer to the adaptive significance of the trait in question, that is, to the effects which led to the evolution of the trait; proximate explanations on the other hand, refer to the immediate physiological mechanisms by which the trait comes about. In the case of the present example, it is clearly quite correct for Grene to argue that the activities of the mind cannot be reduced to its proximate determinants. Comparing modern sociobiology with Hobbes' attempt to "assimilate 'moral' into 'natural' philosophy", Grene writes:

> It was a bold attempt and most unpopular, since like sociobiology it aimed at the reduction of all standards, purposes or duties - all to which we owe allegiance - to the blind bombardment of particles by one another (ibid, p.218).

The difficulty, of course, is that "the blind bombardment of particles by one another", even if an accurate picture of human brain activity (which it is not), is a proximate and not an ultimate explanation of mental activity. As such, it must itself be reducible to a further level of explanation, namely that of ultimate causation. All that proximate causes tell us about a phenomenon, is how it works, and not why it works in the way it does. Thus, in the case of mental activity, the most neurophysiology (which is what Grene is really talking about) could explain, for instance, about uttering a particular sentence on a particular occasion, is the physiological changes which have to occur in order for that utterance to be made. To understand *why* the utterance was made, that is its motivation, it is necessary to appeal to an ultimate explanation. That is to say, it is necessary to pose the question, what was the adaptive significance of the neurophysiological mechanism which was responsible for that utterance being made on that occasion in the environment of evolutionary adaptedness? Or, in the terms of the foregoing discussion, why did it pay the genetic ancestors of the individual in question to interpret certain features of their environment in such a way that this action was prompted on its basis?

What this suggests is not that biological explanations of mind should be abandoned, but, on the contrary, ultimate biological explanations are the *only* admissable explanation of mind. Only by making the assumption, made by Grene in common with conventional social theorists, that the contents of the mind are ultimately arbitrary, is it possible to avoid this conclusion, and, as I have repeatedly stressed, this assumption is

impossible to sustain. Also neatly demonstrated by Grene's misunderstanding, however, is the exact level to which we are justified in requiring explanations to reduce. As I argued in chapter one, and following unavoidably from modern evolutionary theory, ultimate explanations can only ever be given in terms of relative success of the *genes* which underwrite the trait in question. However, the unit whose behaviour determines the success of any particular gene (because it is determined *by that gene*) is, equally unavoidably, the individual organism, or what Dawkins calls the gene's "vehicle" (Dawkins 1982, 1989). In this sense, then, ultimate explanations are inevitably and exclusively concerned with the consequences of genetically determined behaviour of whole individuals.

It would therefore be incorrect to seek ultimate explanation at the level of neurophysiology, but would be equally fallacious to seek it at the level of groups of individuals (as Grene's analysis seems to, and as sociological analysis explicitly does). This is becuase, in the case of neurophysiology, even if direct consequences of particular processes could be examined, the genetic evolutionary consequences could not be known without reference to the behaviour of the entire organism: genes for a perfectly functioning heart will not spread, if the organism also possesses genes for self-destruction at the point of reaching sexual maturity. Similarly, groups of individuals constitute an environment in which specific actions of individuals take place, and will to that extent exert an influence over the evolutionary success of any individual action. But precisely because the actions of other individuals are determined by their own genetic make-up, and not that of the individual in question, the behaviour of these others

cannot determine the spread of that individual's genes. Put another way, other individuals do no more than to provide an environment in which some individual actions will be more evolutionarily efficacious than others. This, I should add, also includes the case of genetic relatives, for even in this case, genes can only exert a direct influence over the behaviour of the individual who bears them, whether this behaviour is selected to be altruistic towards relatives or not.

From this reasoning it is clear that it is quite incorrect to seek ultimate explanations of behaviour at any level other than the behaving organism, whether this be at the neurophysiological or the sociocultural. Also following from this, the argument that human action in modern environments should be considered at a collective level because such action produces remote consequences ("the unintended consequences of action"), can be seen to be fallacious. This is because whilst the consequences of actions are indeed often remote, and quite different from what they would have been in the environment of evolutionary adaptedness, this fact does not alter the motivation of the action, which as we have seen, must be regarded as fundamentally biological. Thus, such remote consequences must be treated as part of the logical category of the *environment*, rather than as forming part of the action's motivation, which is always individual, and hence amenable to ultimate evolutionary explanation. In this way, remote consequences of actions will certainly affect the outcome of the actions of others; but, what is essential to realize in this connection, is that they do so indirectly, by affecting the environment in which motivated individuals act, and so cannot be treated as determinants of action in a motivational sense.

Evolution, the Nature of Mind and the Meaning of Action

If, as the foregoing arguments suggest, we are correct in requiring explanations of human behaviour to reduce to the biologically motivated individual, the question which must now be addressed is, what precisely is the nature of the mind which is thereby suggested? First of all, we can say with certainty that we are not dealing with the sort of entity implicitly assumed by most social theorists, or that stated explicitly, for example,by Levi-Strauss, or by Marjorie Grene. Here, as we have seen, mind is understood as a principally facilitating agency, which is crucially devoid of all content. The content of the actions which it facilitates are arbitrary, and hence only explicable with reference to exterior factors. Similarly, we can be equally certain that we are not dealing with the entity proposed by the sociobiological theorists discussed in chapter two. Here, by contrast, mind does possess a biologically determined content, but is equated with behaviour, and thus is assumed to operate in an environmental vacuum, with the effect that behaviour which is not adaptive cannot be regarded as behaviour and so can have no mental origin.

Suggested by the arguments presented here, however, is a model of mind which, in contrast to the sociobiological model, actively interprets its environment, but unlike the sociological model, interprets this environment on the basis of a biologically determined set of meanings which motivate specific actions. We know that the meanings which are attributed to the environment must have had some adaptive significance in the environment of evolutionary adaptedness, as must the actions

which are motivated on that basis. Thus, although the consequences of actions performed in modern environments may be maladaptive, they must possess a *deep significance* for the actor which would have motivated adaptive actions in the original environment.

The mind, therefore, which these observations imply, must not only contain a common biologically determined set of "deep" meanings, which are attributed to features of the environment according to certain innate adaptive principles, but must possess mechanisms linking these meanings with the performance of specific actions. The implications of this is that actions observed in a modern environment, which are clearly novel or maladaptive, must be analysed in terms of their *deep meaning* or *deep content*, and the mechanisms by which this is related to its *manifest* or *surface* content. Only by analysing the link between the two levels, will it be possible to give an adequate evolutionary account of the motivation of action, whilst at the same time recognizing that actions do not neatly reflect the predictions of crude and rigid biological determinism. Since, however, such meanings and motivational mechanisms only exist in the biological human mind, it is here that the analysis of the significance of action must occur, if it is to avoid the various fallacious reductions which I have discussed.

In the chapters which follow, therefore, I shall offer a detailed account of the model of mind which I believe best meets these demands, and show how its adoption provides the tool necessary for the adequate evolutionary explanation of apparently inexplicable human actions. The

model I have in mind is that advanced by classical psychoanalytic theory, and it is to that I now wish to turn.

CHAPTER FIVE

The Biological Foundations of Psychoanalysis

Since Freud first advanced the fundamental concepts of psychoanalysis, there has been no shortage of self-styled "Freudians", ready to adopt and adapt these concepts, and to defend against vociferous critics such unpopular concepts as the Oedipus complex, repression, penis-envy and the unconscious. Similarly, there has been no shortage of critics, who, with similar enthusiasm, have attempted to demolish psychoanalysis from every angle, apparently stopping at nothing to expose Freud and his followers as ludicrous charlatans and confidence tricksters (see for example, Masson 1984; Eysenck, 1986). Yet there seems to be one aspect of Freud's work, on which supporters and critics have been firmly united: that is, that his speculations on the evolutionary history of the human species range from the dubious to the patently absurd. Writers as diverse as Hans Eysenck (1986) - who, amongst his other questionable beliefs, thinks that Pavlov made a greater contribution to understanding human psychology than Freud - and Ernest Jones (1957), Freud's biographer and principal English follower, are essentially at one in their rejection of Freud's evolutionary claims. So strong has been the rejection of this particular strand of Freud's work, that some analysts have gone so far as to attempt to explain Freud's interest in evolutionary explanation in psychoanalytic terms; E.R.Wallace (1983), for example, tries to account

for the central argument of Totem and Taboo, in terms of Freud's own father-conflict.[1]

It is perhaps not altogether surprising that the vast majority of Freud's followers and (re)interpreters have chosen to draw a tactful veil over his phylogenetic and anthropological speculations in general, and in particular what Serge Moscovici (1985) calls "the black books of Dr Freud".[2] After all, Freud's biological thinking was based upon two central premises, Haeckel's *biogenetic law*, and Lamarck's belief in the inheritance of acquired characteristics (see Ch 1 above), which even in his own life-time were generally known to be incorrect. Ernest Jones reports having "begged" Freud to omit references to Lamarckian inheritance in Moses and Monotheism because "no responsible biologist regarded it as tenable any longer" (Jones, 1957, Vol. 3, p.313). What is perhaps more surprising, however, is the absence - until very recently - of any serious attempt to align psychoanalysis with a correct understanding of evolutionary biology.

In part this omission must be related to Freud's own apparently environmentalist bias. Freud was fond of emphasising the separation of psychology from biology in his clinical writings, but usually it seems for fear of the kind of neurophysiological reductionism discussed in the previous chapter. He writes for example:

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[1] Interestingly, Eysenck (1986) appears to agree with him, and thus performs the not uncommon intellectual trick of using psychoanalytic theory to discredit psychoanalytic theory.

[2] The "black books" are Totem and Taboo, The Future of an Illusion, Civilization and its Discontents, Group Psychology and the Analysis of the Ego, and Moses and Monotheism.

> We have found it necessary to hold aloof from biological considerations during our psycho-analytic work and to refrain from using them for heuristic purposes. so that we may not be misled in our impartial judgement of the psycho-analytic facts before us (Freud, 1913b, S.E. 13, pp. 181 - 82).[3]

This is amplified, when he writes in his Introductory Lectures on Psycho-analysis:

> ...psycho-analysis must keep itself free from any hypothesis that is alien from it, whether of an anatomical, chemical or physiological kind, and must operate entirely with purely psychological auxiliary ideas (Freud, 1916-17, S.E.15, p.21).

It is true also that it was, in large measure, the biologism of such notorious figures as Alfred Adler, G.Stanley Hall, and C.G.Jung which led to their departure from the orthodox psychoanalytic movement. It is important, however, to place these observations in perspective. The importance of phylogenetic considerations in Freud's work cannot be doubted, and indeed, as I shall go on to argue, are fundamental to a coherent understanding of his theory; he insisted, however, that consideration of such factors should not be achieved at the expense of giving adequate weight to ontogenetic factors. For Freud, phylogeny and ontogeny were simply two sides of the same problem:

> I fully agree with Jung in recognizing the existence of this phylogenetic heritage; but I

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[3] This and all susequent references to the work of Sigmund Freud are taken from The Standard Edition of the Complete Psychological Works of Sigmund Freud, (S.E.), giving volume and page number. The year refers to the date of original publication.

> regard it as a methodological error to seize on a phylogenetic explanation before the ontogenetic possibilities have been exhausted. I cannot see any reason for obstinately disputing the importance of infantile prehistory while at the same time freely acknowledging the importance of ancestral prehistory. Nor can I overlook the fact that phylogenetic motives and productions themselves stand in need of elucidation, and that in quite a number of instances this is afforded by factors in the childhood of the individual. And finally, I cannot feel surprised that what was originally produced by certain circumstances in prehistoric times and was then transmitted in the shape of a predisposition to re-acquirement should, since circumstances persist, re-emerge in the experience of the individual (Freud, 1918, S.E. 17, p.97).

It is clear therefore, that Freud's emphasis on ontogeny, which in part resulted in his split with Jung, reflected not the kind of environmentalist preoccupation of many of his successors, but a particular understanding of the relationship between phylogeny and ontogeny. As Frank Sulloway has put it:

> As a committed psychobiologist in his overall approach to the human mind, Freud knew that proximate (that is, psychological and physiological) as well as ultimate (evolutionary) explanations were necessary for a complete theoretical understanding of the subject (1980, p.391).

It is precisely the nature of this understanding between proximate and ultimate causation, which I propose to elaborate in this chapter, which

secures the contribution of classical psychoanalytic theory to the problems posed by modern evolutionary theory.

The general failure of psychoanalytic theorists to take seriously Freud's attempts at ultimate-causal analysis, may also be related to the therapeutic and generally ameliorative aims which psychoanalysis has adopted. In part this is because evolutionary speculations seem academic and remote from the perspective of the consulting room. But perhaps more significant than this - and relevant to the not inconsiderable use which social theorists have attempted to make of psychoanalysis - are the apparent limits to optimism set by phylogenetic factors. After all, pathological defences brought about by harsh familial experiences, or by oppressive political regimes, could, by therapeutic proceedures - individual or collective - in principle be relieved. Concentration, however, upon the evolutionary history of these defences, implies a constitutional factor, not so readily amenable to environmental interference, and perhaps the acceptance of the limits of human happiness. Freud was well aware of this fact, as is show by a remark to a student in the 1930s:

> My discoveries are not primarily a heal-all...[They] are the basis for a very grave philosophy. There are very few who understand this, there are very few who are capable of understanding this (Quoted in Sulloway, 1980, p.439).

So dominant has this environmental emphasis been in the history of psychoanalysis (although, as I shall show later, the last decade has perhaps seen the beginning of a a reversal of this tendency) that it is

tempting to regard that history as the final vindication of Freud's contention that he had dealt the final blow to human narcissism: a blow, it should be added, which was ducked by even his closest followers. When the details of Freud's understanding of the relationship between phylogeny and ontogeny are closely examined, however, it becomes clear that such environmentalist developments do not merely obscure the relevance of Freud's thought to modern evolutionary biology, but allow psychoanalysis to fall into exactly the same theoretical traps as the dominant traditions of twentieth century social theory.

In this chapter, therefore, I wish to consider a number of key developments within psychoanalytic theory, and in the light of these emphasise how Freud's own formulation, informed as it was with a flawed theory of evolutionary biology, nevertheless proposes a model of the human mind fundamentally compatible with the theoretical and empirical demands of modern evolutionary theory. In particular, I wish to consider the implications of the environmental assumptions underlying the formulations of the various neo-Freudian schools of ego-psychology, British object-relations theory, and critical psychoanalysis.

Ego-Psychology , Adaptation and the Source of Motivation

One of the most significant post-Freudian attempts to develop psychoanalytic theory - especially from the point of view of theoretical

sociology[4] - was the emergence in the United States of "ego-psychology". The principal figure in the development of this approach was Heinz Hartmann, although Hartmann's approach is perhaps best known through the work of Erik Erikson (e.g. Erikson, 1950). To the reader familiar with the work of Hartmann (e.g. 1939, 1964) it may appear somewhat surprising that I have chosen to begin my critique of the environmental bias in psychoanalysis here. After all, in many ways, Hartmann's work appears to be the most explicitly "Darwinian" of all psychoanalysts', concerned as it is with the central problem of "adaptation"(Hartmann 1939), whilst apparently retaining the essence of Freud's formulation. As we shall see, however, despite this emphasis, Hartmann's theory in fact makes assumptions fundamentally incompatible with adaptation properly understood, and in crucial respects, offers an account of motivation which is the exact opposite of that which such a concern suggests.

Hartmann's avowed intention is to expand what he regards as Freud's principal interest in the pathological, and continue his early concern, elaborated in his Project for a Scientific Psychology, (1895, S.E.1) to produce a "general psychology" (Greenberg & Mitchell, 1985, p.238). He is thus concerned to introduce into psychoanalytic theory an account of normality, and in particular, to emphasise the role of *reality* in normal - as opposed to pathological - states. It is important for our purposes to explain exactly how this is achieved, since although

[4]It is perhaps significant that Heinz Hartmann had undergone "intensive study" with Max Weber (Greenberg & Mitchell, 1983, p237).

Hartmann regards his analysis as essentially a development of Freud's view, it does, in fact represent a fundamental divergence.

Hartmann - as the label "ego-psychology" suggests - offers a modification of the classical view principally in terms of the nature of the ego. In Freud's metapsychology, the ego is conceived as an agency motivated by the instinctual demands of the id, but which takes into account the restrictions imposed by reality (and its internalized infantile representative, the super-ego). The importance of this single source of motivation, and the mediating role of the ego, is illustrated in the following account by Freud:

> If the id's instinctual demands meet with no satisfaction, intolerable conditions arise. Experience soon shows that these situations of satisfaction can only be established with the help of the external world. At that point the portion of the id which is directed to the external world - the ego - begins to function. If all the driving force that sets the vehicle in motion is derived from the id, the ego, as it were, undertakes the steering , without which no goal can be reached. The instincts in the id press for immediate satisfaction at all costs, and in that way they achieve nothing or even bring about appreciable damage. It is the task of the ego to guard against such mishaps, to mediate between the claims of the id and the objections of the external world...In so far as it tames the id's impulses in this way, it replaces the pleasure principle, which was formerly alone decisive, by what is known as the 'relaity principle', which, though it pursues the same ultimate aims, takes into account the conditions imposed by the real external world (1926, S.E. 20, p.).

Hartmann, however, offers a conceptualization of the ego, which rather than being monistically motivated by the id, is *dualistically* motivated by both the id *and* the demands of reality. This modification is made possible in Hartmann's scheme by his employment of the concept of adaptation, and his conception of the ego as a fundamentally *adaptive* agency (Hartmann 1939). It is important, however, to be clear about the exact sense in which this term is used. For Hartmann, the ego is considered "adaptive" in the sense that it facilitates the individual's physical survival. This is achieved by allowing the individual to "fit in" with environments of an "average expectable" range (1939, p.24), and:

> ..the crucial adaptation a man has to make is to the social structure, and his collaboration in building it (1939, p.31).

Because Hartmann regards *fitting in* with the social structure as a primary adaptive requirement of the individual, he is able to conceptualize the ego in the way he does. By allowing the environment to directly motivate the activities of the ego, Hartmann considers that the environment - essentially the *social* environment - produces a "partial domestication of the pleasure principle" (1964, p248). In this way, Hatmann's scheme essentially allows the demands of reality to *define* the individual's objects of pleasure, and thus effectively closes the gap between the two classical concepts of reality and pleasure principles:

> ...pleasure premia are in store for the child who conforms to the demands of reality and

> of socialization; but they are equally available if this conforming means the acceptance by the child of of erroneous and biased views which the parents hold of reality (1964, p.258).

Whilst Hartmann does not himself lay particular emphasis upon the extent to which this formulation differs from Freud's, it is important for the present discussion that the differences are emphasised. By allowing a direct motivational route from the demands of external reality to the individual, it is possible for Hartmann to regard the ego as possessing spheres which are entirely conflict free. Indeed, it is this potential for the absence of intra-psychic conflict which allows ego-analysts - most notably Erikson (1950) - to equate mental health with a successful adaptation (in Hartmann's sense) to reality. For Freud, on the other hand, conflict is inherent in the very nature of the ego, which, in fact, owes its existence to the fundamental gulf which exists between the pleasure and reality principles. Moreover, Freud's ego is only motivated to act as the result of the frustration of the pleasure principle by the reality principle. Thus, in Freud's formulation, the reality principle is never motivating in any direct sense, but exerts its influence by means of forcing the ego into a compromise.

It is therefore in direct contradiction of Freud's whole model of psychic economy to suggest that the reality principle can absorb the psychical functions of the pleasure principle by redefining the content of pleasure. In Freud's words:

> Actually the substitution of the reality principle for the pleasure principle implies no deposing of the pleasure principle, but

> only a safeguarding of it. A momentary pleasure, uncertain in its results, is given up, but only in order to gain along a new path an assured pleasure at a later time (1911, S.E.12, p. 223).

It is possible to see from this quotation, not merely the crucial respect in which Hartmann's formulation differs from Freud's on the matter of motivation, but also that Freud's own formulation is in fact much closer to the modern evolutionary conception of motivation which I advanced in the previous chapter. To see why this is so we need to notice the divergence between Hartmann's notion of adaptation and that of modern evolutionary theory. Whilst from the point of view of the latter, adaptation is, as we have seen, a matter of ultimate reproductive success, for the former it is essentially reduced to the need for individual physical survival. Whilst physical survival is, up to a point, an obvious prerequisite of reproductive success, it is far from sufficient. It is, for example, quite possible to imagine an individual quite adept at maintaining his own physical survival, but hopelessly unsuccessful at reproduction. The more basic problem with Hartmann's view however, is that adaptation is equated with an external source of motivation. Thus, for Hartmann, the most "well-adapted" individual is one who has subordinated his own internal biologically based motivation to the demands of external reality and actually absorbed these demands as his own source of pleasure. When it is realized that, in evolutionary terms, external reality is essentially the inclusive fitness interests of other individuals, it is possible to see that Hartmann offers a model of motivation identical, from an evolutionary point of view, to that of conventional social theory.

Freud's conception, by contrast, is founded upon a basic and irreconcilable tension between the deep biologically rooted source of motivation (the pleasure principle), and the conflicting frustrating demands of the reality principle. The crucial point about Freud's formulation, then, is that the motivation of all action is ultimately and exclusively derived from within the biologically evolved constitution of the individual, and that whilst this exists in a perpetual state of tension with the demands imposed by others, it is never subordinated to these. In this way it can be seen that Freud's theory of mind possesses the first and most basic prerequisiste of an evolutionarily adequate model: a biologically determined, culturally independent source of motivation.

Object-Relations, Pleasure and the Source of Phantasy

Another significant development in psychoanalytic theory, both from a clinical point of view and from the point of view of social theory, was the emergence of the British object-relations school, and in particular, the work of such men as W.R.D.Fairbairn (e.g.1952), Harry Guntrip (e.g.1973), and D.W.Winnicott (e.g.1958). For present purposes I shall concentrate mainly on the modifications to the classical position proposed by Fairbairn, since these illustrate the essential divergence between the two approaches.

Not unlike the ego-psychology of Hartmann, the starting point of the object-relational approach is the premise that Freud's orignial

formulation suffers from its reliance upon endo-psychic pleasure-seeking drives as the sole motivating factor in mental life. Unlike Hartmann's reformulation of the *source* of pleasure, however, Fairbairn attempts to "rescue" psychoanalysis from this failing, by proposing an alternative to Freud's model which does away with the notion of pleasure altogether. Thus, Fairbairn proposes a modification in Freud's economic view which regards the libido as "not primarily pleasure-seeking, but object-seeking" (1952, p.155).

This alteration of Freud's notion of pleasure has a number of important implications which are recognized explicitly by Fairbairn. The first is the necessary reformulation of the relationship which Freud conceived between *structure* and *drive*. Throughout his metapsychological writings, Freud maintained a theoretical distinction between three fundamental dimensions of psychoanalytic enquiry: the *dynamic*, the *topographical* and the *economic* points of view. The first refers to the constant exertion of contradictory intra-psychic forces; the second to the presence of distinct functional sub-systems, and the third to the existence of a psychical energy, capable of "increase, decrease and equivalence" (Laplanche & Pontalis, 1973, p.127), which "powers" all mental activity.

Whilst this distinction makes little practical difference in the case of the id, in the case of the ego, it produces an important distinction between the processes of which the structure is capable, and the energy

which motivates the carrying out of these processes. [5] Thus in Freud's model, the libido is conceived as initially directionless energy, which, as the result of specific processes, secondarily becomes attached to objects through the activity of the ego. This, as I shall emphasise shortly, does not deny the importance of specific object-characteristics in the satisfaction of libido, but merely stresses the facilitating nature of objects in achieving pleasure, which remains the *aim* of the libido.[6]

Fairbairn, by contrast, regards the ego as inherently energized. Thus, rather than responding to the energy supplied from the id by cathecting it to objects which are acceptable to all conflicting psychical parties, the ego possesses the energy, and so is primarily motivated to form object relationships from the outset. The metapsychological consequences, therefore, of introducing a primarily motivated ego, is the explicit abandonment of the separation of topography and economy, and thus, the necessary implicit abandonment of an inherently *dynamic* conception of mental life. As Guntrip has put it:

> The only escape from a dualism of radically opposed structures is to banish the term "id", and reserve "ego" to denote the whole basically unitary psyche with its innate potential for developing into a true self, a whole person (1973, p.41).

A further consequence of the reversal of Freud's object/pleasure relationship is Fairbairn's modification of the classical theory of

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[5]This distinction is, of course, strictly analytical: the structure could not be observed without the enrgy, nor vice versa, and this is why there is always a dynamic point of view.

[6]See the discussion of the relationship between source, aim, object and pressure in "Instincts and their Vicissitudes" (Freud, 1915, S.E.14).

psycho-sexual development. Coherent with his view of the ego as primarily motivated to form object relationships, Fairbairn replaces Freud's theory of psycho-sexual stages with a sequence of object-relational stages. Thus Freud's scheme of oral-anal-phallic-gential, is replaced with immature dependence - transitional - mature dependence, the first referring to a state of totally undifferentiated identification with the mother, and the last to the ability to experience oneself as entirely separate from others. Whilst in modern circumstances, Fairbairn believes that the transitional phase was usually problematic, it is, in principle, he argued, possible to pass through the stages harmoniously:

> In a state of nature the infant would never normally experience that separation from his mother which appears to be imposed upon him increasingly by the conditions of civilization (Fairbairn, 1952, p.109).

It is this peculiarly modern separation between mother and infant, which leads, in Fairbairn's view, to psychopathology.

The cause of psychopathology, and the subject matter of psychoanalysis, is, for Fairbairn, the presence of "internal objects", which result from the interruption of the *natural* development from immature to mature dependence. Because under modern circumstances, the infant experiences the mother as both gratifying *and* frustrating, it internalizes this object in an attempt to control it more effectively. This results in a "splitting" of the ego, and the ultimate basis for all psychopathology: a conflict between the natural urge towards

development, and the unwillingness to surrender the original enticing, yet frustrating object.

What is crucial here, however, and central to all object-relational approaches, is that the source of "internal objects" - and hence all psychopathology - lies in the failure of the external world. This point can be seen most clearly in Fairbairn's version of the Oedipus complex, and infantile sexuality. For Fairbairn, infantile sexuality in general, and the Oedipus complex in particular, arise as secondary defences against a failure to achieve a satisfactory object-relationship, and usually occur as the result of adult seductiveness. The (male) child's antagonistic relationship with the father is likewise a reslut of a relational failure, where the father - like the mother before him - is experienced as both enticing and frustrating. The primary causal significance which Freud accorded to the Oedipus complex, is thus reversed in this scheme, in which it is regarded as secondary and environmentally contingent. In Fairbairn's words:

> ...the role of the ultimate cause [sic.] which Freud alloted to the Oedipus situation, should propertly be allotted to the phenomenon of infantile dependence (1952, p.120).

This quotation reveals more than the technical primacy given by Fairbairn to object-dependence, it also reveals the fundamentally anti-evolutionary nature of his - and subsequent object-relations theorists' - conception of psychoanalysis. The clue to this is in the claim that for

Freud, the *ultimate* cause of psychopathology - and indeed all mental activity - lies in the Oedipus complex.

Fundamental to the centrality of the Oedipus complex in the classical view of both normal and pathological development is the assumption that it is a universal human characteristic, and fundamental to this, is the notion - since Freud's early abandonment of the seduction theory - of phantasy. What is so crucial about Freud's understanding of phantasy, however, is that it is considered to possess a typical content, *irrespective of experience*, which is a product of phylogenetic inheritance, in short, of biological evolution. To express the significance of this, Freud introduced the term "primal phantasy" in 1915, (1915a, S.E. 14, p.269), although it must be emphasised that the idea which the term indicates had already been central to psychoanalytic theory for some twenty years.

Time and time again in Freud's work do we find references to this fundamental idea. In discussing the "sexual theories of children", for example, he writes:

> ...it is not owing to any arbitrary mental act or to chance that those notions arise, but to the necessities of the child's psycho-sexual constitution; and this is why we can speak of the sexual theories of children as being typical and why we find the same mistaken beliefs in every child whose sexual life is accessible to us (1908, S.E.9, p.215).

And again, in the "Wolf Man" analysis:

> Wherever experiences fail to fit in with the hereditary schema, they become remodelled in the imagination[...] It is precisely such cases that are calculated to convince us of the independent existence of the schema. We are often able to see the schema triumphing over the experience of the individual; as when in our present case the boy's father became the castrator and the menace of his infantile sexuality in spite of what was in other respects an inverted Oedipus complex. (1918, S.E. 17, pp.119-20).

Indeed, it should not go unremarked that it is precisely to the explanation of the origins of the specific content of primal phantasies that Freud's much maligned, and admittedly Lamarckian, Totem and Taboo (1913, S.E.13) is devoted.

The relationship between the classical model and Fairbairn's, on the matter of object relations is, therefore, essentially one of basic opposition. Whilst Fairbairn sees the processes, and more importantly, the *content* of mental development as profoundly dependent upon actual experiential relationships with the external object world, Freud sees them as basically dependent upon an innate phylogenetic "schema", which actually defines the subjective experience of the object world. Yet it is important to emphasise that in the classical model experience does not give way completely to phantasy. Indeed, on the contrary, the relationship between phantasy and its objects is explicitly theorized by Freud, and can again be seen in contrast to the object-relational view.

As I pointed out above, Fairbairn's abolition of the drive/structure distinction involves a reformulation of the ego as inherently energized and primarily object-seeking, which contrasts with the classical view of the ego as secondarily attaching directionless energy to objects, in the primary service of the pleasure principle. It is tempting to draw a purely descriptive parallel here between the object-relational approach and the sociobiological view outlined in chapter three. In both cases the failure to distinguish analytically between motivation and the object of motivation leads to a wholly static conception of the environment, in which motivated variations are implicitly ruled out. The classical view by contrast, in making a fundamental distinction between motivation (that is *pleasure*) and the object of motivation, is able to account for variation without collapsing into biological arbitrariness.

To see how this is so, we need to consider Freud's formulation of the relationship between objects and instincts:

> The object of an instinct is the thing in regard to which or through which the instinct is able to achieve its aim. It is what is most variable about an instinct and is not originally connected with it, but becomes assigned to it only in consequence of being peculiarly fitted to make satisfaction possible (1915c, S.E.14, p.122).

The importance of this quotation is in the fact that it demonstrates that in the classical view, the objects of motivation are neither strictly determined by biology, but at the same time are not completely

arbitrary. Instead, objects are chosen because they are "peculiarly fitted" and so must possess some intrinsic quality which suits them for the task, but not to the total exclusion of all others. In terms of the foregoing argument, therefore, we may say that in the classical view, external objects are first chosen because they best resonate with the contents of primal phantasies.

We are now in a position to see how precisely the classical metapsychology - as opposed to the object-relational - meshes with the evolutionary conception of object relations as it has been expressed so far. We have seen that from the classical point of view, objects are chosen because of an intrinsic *suitability*, rather than some unique quality. In other words, there are, in principle, a variety of objects capable of satisfying the instinct. The importance of this observation, however, is that in the environment of human evolutionary adaptedness, there must have been fewer possible objects capable of exhibiting this "peculiar fit", and satisfying the instinct, than there are in modern environments. Put differently, the level of discrimination between suitable and unsuitable objects which we evolved, was adequate for a primal hunter-gatherer environment, and the objects which were chosen on its basis must in general have produced adaptive consequences. Under modern circumstances, however, it is entirely plausible that there are many novel objects which are capable of instinctual satisfaction, but, because we have not yet evolved the means to discriminate against them, produce maladaptive consequences.

It seems, therefore, that the metapsychological conception of the relationship between biology and the external world which is contained in the classical theory of object relations is entirely in accordance with the demands of evolutionary explanations, as outlined earlier. In particular, the notion of primal phantasy, and the separation of object and instinct, characteristically dropped by object-relational reformulations, provide the link between the biologically rooted source of motivation discussed in relation to ego-psychology above, and the basic problem of the motivation of behavioural variation. This link is strngthened, however, when the picture is broadened to include the classical formulation of psycho-dynamic processes, and in particular the basis of *repression*.

Critical Psychoanalysis and the Basis of Repression.

A third group of psychoanalytic revisionists, who have been particularly influential in the adoption of psychoanalytic ideas by social theorists, are the "critical" psychoanalysts exemplified by such men as Wilhelm Reich, Erich Fromm and especially, Herbert Marcuse. Although these writers have all contributed complex and differing Freudian-Marxist syntheses, I am concerned here only with the particular relationship conceived bewteen the individual's biological constitution and society, and the way in which this formulation differs from Freud's own. I shall therefore concentrate upon Marcuse's formulation (1955), as his conceptual revision makes the divergence most clear.

Unlike ego-psychology and object-relations theory, both of which Marcuse is especially critical, he begins his analysis with the literal acceptance of Freud's late instinct theory. Marcuse is particularly unusual in this respect, because not only does he adopt the concept of libido ("Eros"), but also the highly unpopular and speculative notion of the death instinct ("Thanatos"). Marcuse's analysis differs too from his fellow Marxist-Freudian Wilhelm Reich, in that whilst Reich's ideal conception of society is one in which the repression of instincts is entirely absent (an idea which Reich followed to the point of insanity and imprisonment), Marcuse accepts that a "basic" level of instinctual repression is both desirable and necessary (Marcuse 1955, p.38). Where Marcuse differs radically from Freud, however, is in his additional notion of "surplus repression".

This concept is introduced to rectify what Marcuse sees as Freud's failure to adopt an adequately historical perspective, and refers to "the additional controls arising from the specific institutions of domination" (1955, p.46). Thus instinctual repression, beyond the theoretical minima of basic repression, is correlated with the fundamentally economic requirements of domination which obtain in the society in question:

> Within the total structure of the repressed personality, surplus-repression is that portion which is the result of specific societal conditions sustained in the specific interests of domination. The extent of this surplus-repression provides the standard of measurement: the smaller it is, the less repressive is the stage of civilization. The distinction is equivalent to that between the

> biological and the historical sources of human suffering (1955, pp.87-88).

The notion of surplus-repression as an historically relative phenomenon is developed in Marcuse's other major conceptual modification of Freud, his reformulation of the nature of the *reality principle.* Marcuse reads Freud's conception of the reality principle as referring in general to the requirements of civilized existence. Because of his insistence, however, that surplus-repression is historically specific, Marcuse replaces Freud's single abstract conception of reality with the notion of a multiplicity of possible reality principles, the contents of which are determined by the economic infrastructure of the society. Under capitalism, the specific form of the reality priciple is termed by Marcuse, the "performance principle", under the rule of which "society is stratified according to the competitive economic performances of its members" (1955. p.60).

The (surplus) repressions of instinct which are brought about under the performance principle, in Marcuse's view, leads to the sublimation of potentially pleasurable libido into activities which are beneficial to the maintenance of the economic status quo. Even the sphere of sexual activity itself, under the performance principle, is subjugated to the economic demands of the capitalist system; sexual energy which is not diverted into work, is distorted and unnaturally focused into restrictive genitality, rather than into its natural pre-genital polymorphous forms. Marcuse's vision is the abolition of all such restrictions of libido so that:

> The free development of transformed libido within transformed institutions...would minimise the manifestations of mere sexuality by integrating them into a larger order, including the order of work (1955, p.202).

The performance principle, therefore, is responsible for the specific forms of instinctual repression under capitalism, and since its content is directly determined by the demands of the economic infrastructure of capitalism, Marcuse's theory involves an effective equation of intra-psychic repression with *economic* repression. Thus, the abolition of capitalism will bring with it the 'withering away' of the performance principle, and hence the abolition of surplus repression and an accompanying resurgence in the primacy of the instincts.

Leaving aside the various empirical difficulties with Marcuse's case, not to mention the difficulties raised by his retention of the death instinct, it is, for our purposes, important to notice the respects in which Marcuse's approach to repression and reality differs from Freud's. Whilst Marcuse sees the origin of surplus-repression (which for him is ultimately the only significant from of repression) in the economic infrastructure, Freud unequivocally traces the origin of repression - and most importantly the contents of what is repressed - to a biological evolutionary root. This is a fact which is frequently overlooked, even by otherwise accurate accounts (e.g.Bocock 1976), but which is in fact essential to the coherence of Freud's theory of repression.

To see how important this is to Freud's formulation, it is essential to grasp the distinction which he makes in his metapsychological account between "primal repression" and "repression proper":

> We have reason to assume that there is a *primal repression*, a first phase of repression, which consists in the psychical (ideational) representative of the instinct being denied entrance into the conscious. With this a *fixation* is established; the representative in question persists unaltered from then onwards and the instinct remains attached to it...
>
> The second stage of repression, *repression proper*, affects mental derivatives of the repressed representative, or such trains of thought as, originating elsewhere, have come into associative connection with it, On account of this association, these ideas experience the same fate as what was primally repressed. Repression proper, therefore, is actually an after-pressure. Moreover, it is a mistake to emphasize only the repulsion which operates from the direction of the conscious upon what is to be repressed; quite as important is the attraction exercised by what was primally repressed upon everything with which it can establish a connection. Probably the trend towards repression would fail in its purpose if these two forces did not co-operate, if there were not something previously repressed ready to receive what is repelled by the conscious (1915b, S.E. 14, p. 148, *italics in original*).

It is possible to see from this quotation that, for Freud, everything which the adult mind subsequently represses, only meets this fate because of an "associative connection" with the content of the primally repressed. To fully comprehend this relationship, however, it is

necessary to consider the role of the super-ego in repression, and its relationship with phylogenesis.

The most important point to note here is that repression proper presupposes the existence of the super-ego:

> Since we have come to assume a special agency in the ego, the super-ego, which represents demands of a restrictive and rejecting character, we may say that repression is the work of this super-ego and that it is carried out either by itself or by the ego in obedience to its orders (1933, S.E.22, p. 67).

The ontogenetic emergence of the super-ego as the internalization of certain parental prohibitions, however, presupposes the existence of primal repression, because, as we have seen, it is the content of primal repressions which determine the content of subsequent repressions through their "associative connection". What this means then, is that the content of the super-ego is itself explicable as a primal repression, and so ultimately explicable with reference to phylogenesis. Thus, Freud' s Lamarckian interpretation:

> The super-ego, according to our hypothesis, actually originated from the experiences that led to totemism...The experiences of the ego seem at first to be lost for inheritance; but, when they have been repeated often enough and with sufficient strength in many individuals in successive generations, they transform themselves, so to say, into experiences of the id, the impressions of which are preserved by heredity. Thus the id, which is capable of being inherited, are

> harboured countless egos; and, when the ego forms its super-ego out of the id, it may perhaps only be reviving shapes of former egos and be bringing them to resurrection (1923, S.E.19, p.38).

Once allowance has been made for Freud's Lamarckian biology, it can be seen that he is making the crucial point that the super-ego, the agency whose contents are responsible for repression proper, does in fact have an evolutionary history, and so its contents - and hence the contents of what is repressed - is not environmentally contingent, but ultimately biologically determined. As Sulloway has put it:

> Freud's notion of the superego constitutes an important conceptual watershed between his two formal categories of primal and secondary repression [*repression proper*]. The superego arises as the last of the great primal repressions and, like them, is reducible to an organic process founded in the ancestral experience of the race (1980, p.375).

What is crucial from our point of view, and what distinguishes Freud's conception of repression from that advanced by Marcuse, is that for Freud, the content of what is repressed has an evolved biological basis, whilst, for Marcuse, it is contingent upon the particular mode of production which happens to be in operation at the time. Thus, exactly as with the case of primal phantasy, which, as we have seen, provides the basic "schema" for the content of the developing child's phantasy life, so primal repression provides the basic schema for the content of what is repressed, and internalized as the super-ego.

Besides the obvious logical superiority of Freud's version - accounting as it does for the initial act of repression, which is presupposed by subsequent repressions - it is important to notice, once again, how his assumption of a basic evolved *content,* presents a view which is coherent with the demands of evolutionary explanation. This is because, for repression to have an evolutionary basis - which if it exists as a process it must - it is necessary to assume that the effects of repression (in the environment of evolutionary adaptedness) were adaptive. If it were assumed, however, that the content of what is repressed is completely arbitrary it would be impossible to make this assumption for the simple reason that repression is, by definition, a selective process, and so it must be more adapative to repress some things than others. In Marcuse's theory, however, there is no biological basis to the content of repression, and so there is no *a priori* reason why any material should be in any way more likely to be repressed than any other; it depends entirely upon the mode of production of the moment, and not on the inclusive fitness interests of the individual. In Freud's formulation the fact that the content of what is repressed, as well as the *capacity* for repression, has a biological basis, it is possible to conceptualize it as the product of evolution. In Marcuse's case, however, repression must ultimately meet the same fate as culture did for the conventional social theorists in chapter three.

In this chapter, I have argued that the unpopular, but crucial, evolutionary biological assumptions which underlie classical psychoanalytic theory, whilst substantively incorrect, in fact mean that the model of mind which is thereby articulated is essentially in

accordance with that suggested by modern evolutionary theory. So far I have concentrated upon the fact that in the classical model, the mind and its processes possess a biologically determined content which distinguishes it from the purely facilitating model assumed by both conventional social theorists, and a number of key psychoanalytic revisionists. In the next chapter, I wish to turn my attention to the analysis of this content, and consider its adequacy in the light of modern evolutionary theory, and in particular consider the evolutionary significance of the classical formulation of the content of repression and the dynamic unconscious which it assumes.

CHAPTER SIX

Evolution, Deception and the Dynamic Unconscious

As the arguments I have advanced in earlier chapters make clear, the recognition that human beings possess unique characteristics such as symbolic language, the capacity for self-reflection, and most of all consciousness, does not separate them from the evolved biological world. On the contrary, as I have repeatedly suggested, the only ultimate explanation for the possession of these characteristics is an evolutionary one, and therefore one which specifies the adaptive significance of the effects of the trait in question in the environment in which it evolved.

It is perhaps surprising that evolutionary biologists have been, in general, rather negligent of the whole area of human consciousness. It is, after all, as the social theorists frequently point out, what distinguishes human behaviour from that of all other species, and what apparently gives human motivation such a unique character. It is, in particular, what permits human beings to act teleologically, to assess goals and means, and to formulate motives. The question which must be faced, therefore, is what was the adaptive significance of this capacity, and in particular, what can evolutionary considerations tell us about its nature?

Inclusive Fitness and Self-Consciousness

Perhaps the most important point to consider, is that human beings are not merely conscious of their environment, but, crucially, are conscious of themselves. This realization formed the cornerstone of the philosophy of

George Herbert Mead (amongst others) and has, naturally enough, become a central theme in modern social theory. At first sight, the emergence of self-consciousness does not seem difficult to understand in evolutionary terms. This can best be seen by considering the evolutionary implications of Mead's classic statement of the nature of the self (1962). For Mead, the human self is distinguished as unique because it is capable of being both subject and object (1962, pp. 136-7). It is also a fundamentally *social* self, because its capacity for reflexivity is derived from social experience, that is, through "taking the role of the other". Through this process of role-taking, Mead argues that human action proceeds by the monitoring of one's own conduct from the point of view of another:

> The immediate effect of such role-taking lies in the control which the individual is able to exercise over his own response (1962, p.245).

To see how such a tendency could evolve, but also to see its limitations, it is necessary to consider the role which "the other" plays in interactions from the point of view of evolutionary theory. Clearly, from this point of view, 'the other' is not a unitary concept, for the simple reason that some 'others' share more of 'self's' genes than others. Yet, leaving this aside, the point of view of the other will clearly be an important consideration in the success or failure of a given proposed action, since there will always be a significant degree to which the genetic interests of the other will diverge from those of self (even in the case of quite close genetic relatedness). Thus, since an action which is completely in the interests of self, will almost certainly not also be in the interests of other, some internal representation of the interests of other will plainly be advantageous. This is because, a failure to consider the point of view of the other at all, when

pursuing one's genetic self-interest, would inevitably lead to conflict, the negative consequences of which could threaten to outweigh the benefit of the action.

Yet there is one important respect in which Mead's formulation can be seen to constitute a distinct adaptive liability. This relates to the *motivational* totality of role-taking, which is a central feature of Mead's idea. For Mead, lacking the perspective of modern evolutionary theory, role-taking is regarded as a process by which individual orientation to the demands of the other (usually the collective), is achieved by the *absorption* of these demands as part of the self, with the consequence that they effectively become motivationally undifferentiated. Mead is at times quite explicit about this:

> And thus it is that social control, as operating in terms of self-criticism, exerts itself so intimately and extensively over individual behavior or conduct, serving to integrate the individual and his actions with reference to the organized social process of experience and behavior in which he is implicated. The physiological mechanism of the human individual's central nervous system makes it possible for him to take the attitudes of other individuals, and the attitudes of the organized social group of which he and they are members, toward himself, in terms of his integrated social relations to them and to the group as a whole; so that the general social process of experience and behavior which the group is carrying on is directly presented to him in his own experience, and so that he is thereby able to govern and direct his conduct consciously and critically, with reference to his relations both to the group as a whole and to its other individual members in terms of this social process. Thus he becomes not

> only self-conscious but also self-critical; and thus, through self-criticism, social control over individual behavior or conduct operates by virtue of the social origin and basis of such criticism. That is to say, self-criticism is essentially social criticism, and behavior controlled by self-criticism is essentially behavior controlled socially (1962, P.255).

Self-consciousness is thus explicitly equated by Mead with social control, and hence social motivation. In other words, rather than allowing the point of view of others to be taken into account when anticipating a particular course of action in the pursuit of genetic self-interest - as the account given above suggests - Mead is in fact allowing the interests of others to *directly motivate*, rather than to frustrate action, and thus denies the possibility of genetically self-interested actions. As Mead put it:

> Hence social control, so far from tending to crush out the human individual or to obliterate his self-conscious individuality, is, on the contrary, actually constitutive of and inextricably associated with that individuality; for the individual is what he is, as a conscious and individual personality, just in so far as he is a member of society, involved in the social process of experience and activity, and thereby socially controlled in his conduct (1962, p.255).

It would seem then, that self-consciousness of the sort proposed by Mead could not be expected to evolve, since it would have the effect of directly prioritizing the inclusive fitness demands of the other. But what precisely is the form of self-consciousness predicted by inclusive-fitness theory? To answer this question it is necessary to consider certain basic predictions of

inclusive-fitness theory about the nature of interaction, and in particular, the precise circumstances in which the genetic self-interest of others could be expected to be promoted by an individual; in other words, the circumstances for the evolution of altruism.

The Evolution of Self-Decepetion and the Nature of Repression

I have no wish to become involved here in semantic arguments about whether what I am going to discuss can legitimately be called altruism. Obviously, as was demonstrated by the theory outlined in chapter one, to speak of the *evolution of altruism* is contradictory in that altruism, defined as an act which benefits the inclusive fitness of the recipient at the expense of that of the altruist, cannot evolve. What I mean to delineate, therefore, are the circumstances under which *apparently* altruistic actions could evolve, because in reality they enhance the inclusive fitness of the actor.

Apart from the possibility that altruistic acts may be *induced* (Trivers 1985, p.49; Badcock 1986, Ch.3), it is generally accepted by biologists that, following Hamilton's fundamental theorem, there are two essential ways in which apparent altruism can evolve: *kin altruism* and *reciprocal altruism*. The first I have already discussed in chapter one, and refers to the fact that because relatives share genes with calculable frequencies, actions could be selected which enhance the reproductive success of relatives providing that the cost of doing so does not exceed that predicted by the coefficient of relatedness between the two individuals. The second, however, refers to the possibilities of apparent altruism towards unrelated

individuals, and it is principally to Robert Trivers that we owe its theoretical elaboration (Trivers, 1971, 1981,1985).

Trivers reasons that, theoretically, acts of apparent altruism could be selected between unrelated individuals if, at some future time, they produced a reciprocation of equal value. Indeed, he marshalls considerable evidence in support of the view that just such a situation has evolved in many species (e.g.1985, Ch.15). However, he also points out that since the individual who initiates the reciprocal relationship must, by definition, perform the first act before the reciprocation is forthcoming, such relationships are highly prone to exploitation. For this reason, certain characteristics make the evolution of reciprocity more likely:

> During the pleistocene, and probably before, a hominid species would have met the preconditions for the evolution of reciprocal altruism: long lifespan; low dispersal rate; life in small, mutually dependant stable social groups ...; and a long period of parental care (Trivers 1981, pp.10-11).

Yet even under these propitious circumstances, such relationships are capable of being exploited, and since selection will obviously favour those who are most adept at exploiting such situations, can be expected to evolve. At the same time, however, selection will also favour those who successfully resist attempts at exploitation. In this connection Trivers distinguishes between *gross* and *subtle* cheating, where the former refers to the total failure to reciprocate, and the latter to the failure to reciprocate as much as has been received. Whilst it is likely that selection will quickly favour the detection of gross cheats, making total failure to reciprocate an

ultimately unsuccessful strategy, Trivers suggests that the co-evolution of cheating and its detection will produce ever more subtle means of cheating, and ever more subtle means of detection, with the effect that:

> It is the subtlety of the discrimination necessary to detect this form of cheating and the awkward situation that ensues that permit some subtle cheating to be adaptive (Trivers 1981, p. 13).

It is important to point out that the phenomenon of cheating should not be expected to be confined to reciprocal relationships. Given that even in the case of kin relationships there are strictly quantifiable limits upon the degree of apparent altruism which it is adaptive to render, it will clearly also be adaptive to successfully exploit these relationships in exactly the same way as those based upon reciprocity. The significance of this will be discussed more fully later, but for the present it is important to consider the implications of the evolution of subtle cheating and detection, for the nature of self-consciousness.

It would seem inevitable that the co-evolution of subtle cheating and detection must bring with it the evolution of indicators of reliability in reciprocal interactions, and means for accurately detecting those indicators. Clearly, an individual who is predisposed to engage in reciprocal relationships with others who reciprocate will be at a selective advantage over those who engage in such relationships at random. As soon, therefore, as both reciprocators develop some peculiar characteristic, and others develop the predisposition to react favourably to this characteristic in reciprocal exchanges, the co-evolutionary process is set in motion. This

first stage will be followed by the emergence of apparent reciprocators who do not in fact reciprocate (or increasingly, reciprocate less than required), who at first will possess a selective advantage, but only until some means to distinguish between genuine and pseudo-reciprocators emerges. From here, more subtle forms of pseudo-reciprocator will emerge amongst the genuine reciprocators, followed by more subtle means of distinguishing them, and so on.

It has been suggested by a number of authors (eg Trivers 1981, 1985; Alexander 1979; Badcock 1986), that this situation must have had a specific effect upon the evolution of self-consciousness; that is, rather than the continual extension of self-consciousness, its limitation, and facilitation of *self-deception*. This argument follows from the recognition that for self-consciousness to evolve, there must have been some selective advantage to its nature, and that a full awareness of the nature of one's motivations would be not merely unnecessary, but in fact a liability. For example, Alexander writes:

> I suggest that the separateness of our individual self-interesets, and the conflicts among us that derive from this separateness, have created a social milieu in which, paradoxically, the only way we can actually maximise our own self-interest and deceive successfully is by continually denying - at least in some social arenas - that we are in fact doing such things. By conveying the impression that we do not intend to deceive, and that we are in fact altruistic and have the interests of others at heart, we actually advance our own (evolutionary) self-interest. I believe as a consequence that our general cleverness at creating deceptions and detecting them has made it next to

> impossible for individuals to benefit from deliberate deception in ordinary social situations, because of the likelihood of detection and exposure, and, possibly, severe punishment. The result, I believe, is that in our social scenario-building we have evolved to deceive even ourselves about our true motives (Alexander 1979, p. 134).

What this argument suggests is that consciousness appears to have evolved as a sort of self-management system, capable of presenting a set of motivations to others, which are both acceptable and plausible. At the same time, however, it also suggests that consciousness (and self-consciousness) does not play a particularly important role in our actual motivation, of which it will be in our interests to be unaware. As Trivers has put it:

> As mechanisms for spotting deception become more subtle, organisms may be selected to render some facts and motives unconscious, the better to conceal deception. In the broadest sense, the organism is selected to become unconscious of some of its deception, in order not to betray, by signs of self-knowledge, the deceptions being practised. (Trivers, 1981, p.35).

Following this analysis, Christopher Badcock has made the important point that the unconscious which is here described by Trivers is, in important respects, the same as that advanced by Freud, in that it is dynamic, and thus capable of receiving material unacceptable to consciousness by means of *repression*:

> The essentially dynamic nature of Trivers' unconscious is fully apparent...where...he describes the unconscious not merely as

> something apart from consciousness, but as a phenomenon positively engendered by the need to hide certain realities from consciousness - in a word as a consequence of *repression* (Badcock, 1986, p.43, italics in original).

What is perhaps the most important implication of this is that the unconscious predicted by Trivers does not do away with the unacceptable motivation altogether; on the contrary, the motivation remains intact, that is, exerting its influence, but without the awareness of the actor. Indeed, it implies that actions which are in fact deceptively self-serving, will be capable of reformulation by the actor, as in fact correctly reciprocal. I shall develop this point in later chapters, in the context of the example of sexual commitment, but for the present it is important to note how this relates to the Freudian notion of repression and motivation.

Freud's conception of repression, like that implied by Trivers, involves not the abandonment of the unacceptable idea altogether, but rather its denial of access to consciousness. However, it is important to stress that for Freud the repression of a wish also means that the wish in question will not be enacted in that form. This point is not explicitly made by Trivers, who, as the quotation above implies, seems to only apply his argument to the repression of the *knowledge* that an act is self-serving. Clearly, however, this restriction would be adaptively unrealistic, since it would allow individuals to perform patently self-serving actions, whilst simultaneously denying the obvious fact that this is what they are doing. In Freud's formulation, however, a self-serving motivation needs to pass the censor of the ego and super-ego before it can be enacted, and the actor can become aware of what he is doing. What this means then is that

before an action is performed, its motivation must be capable of being consciously formulated by the actor, and crucially, formulated in a way which is acceptable to the demands of the ego and super-ego. To appreciate the significance of this, we need momentarily to remind ourselves of the nature of the ego and super-ego.

We saw in the last chapter that, for Freud, the super-ego is an agency whose origin and content is ultimately explicable phylogenetically, but which develops ontogenetically through the internalization of certain parental demands which are interpreted through the lens of an innate schema. The ego is the agency responsible for the mediation between the conflicting demands of the super-ego, the id (and the dynamically repressed) and external reality. Before an action can be self-consciously performed, therefore, it must be acceptable to the generalized quasi-parental demands of the super-ego, and to the specific demands of reality, whilst still being sufficiently close to the original repressed wish to be motivated at all.

It is possible to see, therefore, that according to the classical psychoanalytic view, but also in accordance with evolutionary expectations, actions are only performed if their motivation can be self-consciously formulated by the actor in such a way as to be acceptable to these various demands. But because we must also assume that only inclusive-fitness enhancing actions can be motivated, the ability to formulate motives in a publicly acceptable way will never be a sufficient condition for an action to be performed. This is because, as Trivers has pointed out, pure reciprocity is not a stable state of affairs, and so those who always act only upon the basis

of publicly approved motivations - that is, for whom all motivation is self-consciously accessible - will systematically lose out to those who are motivated by inclusive fitness demands, not acceptable to other similarly motivated individuals.

This argument suggests, therefore, that repression is not merely a means by which unacceptable motives are kept from consciousness, but also facilitates the performance of actions which are both self-serving, but for which publicly acceptable motives can be formulated. Indeed the focus here falls not upon the act of formulating one's own motives in an acceptable way - which may, of course, be necessary on occasions - but on the fact that they *can* be so formulated, and thus that they may be so formulated by others.

It should be noted that according to the evolutionary view, actions which are performed on the basis of ego and super-ego censorship should still have beneficial effects in terms of inclusive fitness, even though they have been modified by the effects of repression. Whether or not this is actually the case is obviously an empirical question, and one which I propose to address through the examples to be elaborated in subsequent chapters. At present however, it is sufficient to note that the transformations which repressed wishes undergo, before they reach a state which can gain conscious expression, are regarded by Freud to be of a quite specific nature. This means that there are definite paths of connection between the the repressed and its conscious derivitives, and that the two do not become connected by mere accident. Indeed, it is fair to say that the "substitutive-formations" (eg 1926, S.E.20, p.145) which represent the

repressed wish in the conscious, are formed precisely because of an inherent subjective (although unconscious) affinity with the repressed. In this way, it is possible to see that, according to Freud's theory, there is a fundamentally *symbolic* relationship between the content of actions actually performed and the unconscious and repressed wishes which motivate them. This symbolic relationship, however, is of a very distinct kind, and quite different from the relationship conceived by conventional social theorists between action and *sui generis* social forces. Whilst for the latter, as we have seen, action is arbitrarily symbolic of wider social forces, from the psychoanalytic point of view, action is symbolic of unconscious wishes in a motivated and essentially functional sense. In other words, whilst for conventional social theorists, actions are performed ultimately because convention (however theorized) dictates, for psychoanalysis, actions are performed because, by their intrinsic nature, they are able to elicit motivation from specific, biologically rooted, repressed wishes.

As I suggested in the last two chapters, the presence of a novel environment could have the effect of producing actions which are not, in fact, adaptive, but when the theory of repression outlined here is considered, the likelihood of this possibility is greatly increased. This is because repression, as I have expressed it, has the effect of forcing the formation of publicy acceptable substitutes of fitness-enhancing wishes, in the interests of acting in a deceptively reciprocal way. The effect of this, therefore, is that the repressed wish must be capable of undergoing various transformations in order to reach a form in which it can be self-consciously formulated in an acceptable way, and still satisfy the original fitness-maximizing impulse. This suggests a degree of fundamental

lability in the derivitives of the repressed wish, without which the most acceptable "compromise" (e.g.1896, S.E.3, p.170) could not be reached. Freud expressed this in terms of the pivotal distinction made between objects and instincts, discussed earlier in relation to object-relations theory, by which he was able to explain the satisfaction of instincts through objects far removed from those originally cathected. In terms of the foregoing argument, however, it is possible to see that the separation in question, which, as I have argued, is central to the coherence of classical metapsychology, is explicable in terms of the need to produce substitute satisfactions for repressed wishes. In a novel environment, however, the possibilities for substitute formation will obviously be different from those in our original environment, and so it is possible that subjectively acceptable substitutes will be chosen, which lack the essential functional attributes of the repressed wish which they represent. In essence, this is the same as the argument I advanced in the last chapter in connection with objects, but is here widened to include all possible transformations of motivation, of which, as I shall later exemplify, changing the object is but one.

Self-Deception, Insight and the Possibility of Analysis

The argument that self-consciousness has evolved to facilitate the performance of self-serving actions, for which publicly acceptable motives can be formulated, raises a number of questions about the possibility of self-insight, which must be addressed before we can proceed. The first of these, and perhaps the most obvious, is the fact that it is a matter of common experience that patently self-serving motives can be formulated

by actors, and indeed, very often are. At first sight this seems to invalidate the conclusions I have drawn about the nature of repression, and in particular the role of repression in permitting only those actions for which publicly acceptable motives can be formulated. Yet, it should be noted in this connection that the motives which I have suggested should be repressed, are only those motives whose detection by others will confer an adaptive disadvantage to the actor. It might be imagined that all self-serving motives are likely to fall into this category, but in fact this is not the case. The first thing to notice here is that self-serving motivations *per-se*, are not equivalent to motives to cheat. This means that merely pursuing one's own genetic self-interest, in reciprocal or kin relations, should not necessarily be subject to repression, because such actions are publicly acceptable in the sense that they do not involve deception. Thus, to say that one wishes to receive a fair price for an item one is trading, although in some respects self-serving, does not depart from the reciprocally acceptable (indeed, the absence of such a concern might be expected to elicit suspicion on the part of the partner to the exchange).

The second thing to notice, however, is that the the co-evolution of cheating and detection proposed by Trivers suggests that once this system has reached an advanced stage, as presumably it has in the human case, some degree of cheating, or at least a propensity to cheat, should be regarded as normal by participants in reciprocal interactions. Once cheating becomes established as a strategy, we can expect, as I have already pointed out, the detection of cheating to also become established. The next stage is the evolution of a more subtle form of cheating, and, the counter-evolution of a more subtle means of discrimination between

cheats and reciprocators. Yet at each stage, to be a subtle cheat will always be more beneficial than to be a genuine reciprocator, and so it is reasonable to hypothesise that the assumption that any individual is in fact a genuine reciprocator, will become increasingly less reliable. In other words, individuals who do not overtly display some tendencies towards non-reciprocation, could reliably be predicted to be *subtle cheats*, and hence more dangerous than the more obvious cheats around them. Thus, to treat *apparent* full-reciprocators as full-reciprocators becomes an increasingly unsuccesful strategy, whilst engaging in interactions with the more benign, less-subtle cheats becomes increasingly successful, because at least their mode of cheating is easily detectable, and hence controlled.

The most adaptive strategy from this point on, however, is clearly to retain a facade of a tendency towards rather obvious cheating, whilst at the same time developing a strategy for more subtle cheating alongside this. In this way, it is possible to see how the ability to formuate deceptive motives for one's actions is not ruled out by the analysis of repression I have advanced here. Indeed, if this reasoning is accepted, some degree of critical self-awareness would seem to be a pre-requisite of successful self-deception through repression. This argument suggests, therefore, that the general principle that repression allows only those actions for which publicly acceptable motives can be formulated stands, but that what is considered to be "publicly acceptable" should be broadened to include a measure of selfish conduct. Still, however, this does not imply that those selfish motives which are formulated are necessarily to be equated with genuine motives. On the contrary, it is quite feasible that a publicly

acceptable selfish motive (as it were), which is formulated for a given action, in fact disguises its repressed, unacceptable motivation.

This last point brings us to a second, and perhaps less obvious, problem raised by this analysis of self-consciousness, which relates to its implications for the possibility of that analysis itself. If, as I am arguing, the content of all consciousness and self-consciousness is ultimately explicable only in terms of inclusive fitness considerations, then this would seem to preclude the possibility of any account legitimately claiming independent validity. If the only motivation of which I can become self-conscious, is that of which to become self-conscious is evolutionarily adaptive, then the formulation of motivation which I am offering here, cannot be in any way different in quality from that which I am imputing to actors in general. It would seem then, that for the theorist to claim independent validity for his formulation is, in this case, to deny the very premises of that formulation.

The first point to make in reply to this, is that similar criticisms can be made of any attempt to explain human action in terms other than 'pure reason'. This is not to say that action *should* be explained in these terms; on the contrary, such an explanation could not operate independently of the terms of the actor, and would thus not constitute an *explanation* at all. The point which I am making is the familiar one that since social theorists are also social actors, any attempt to theorize social action must also encompass the act of theorizing itself. And, since the act of theorizing implicitly denies the theorized an independent validity - that is, a fully self-evident nature - it can be argued that the act of theorizing therefore

implicitly denies its *own* independent validity, and the self-evident nature which it assumes.

The requirement, therefore, is for the theorist to be able to decouple the act of theorizing from the theorized, and this can generally only be done if it can be shown, minimally, that the conditions responsible for the theorized do not apply to the theorizing, and that those which are responsible for the theorizing are somehow authoritative. In the context of the present argument, therefore, the claim that it is correct to say that the motivation and meaning of an action may in fact be different from that self-consciously attributed to it by the actor could itself be accused of possessing a motivation and meaning different from that which I am attributing to it, namely that it is true. This can be avoided, however, if I am able to show that the conditions which apply to action in general (namely, that its meaning is self-deceptively formulated), do not apply to the act of theorizing about action, and that such theorizing can be formulated in such a way as to secure its independent validity.

But what grounds are there for supposing that such a separation of theorizing from self-deception is possible or legitimate? On the face of it, this seems a most unpromising proposal, since the theory apparently stands or falls on the fact that one's true motivation is obscured from consciousness, and yet, at the same time, it articulates what it claims to be the true basis of motivation. To see how such a separation may nevertheless be possible, it is important to remember that it is only one's *own* motives of which it will be adaptive to be unconscious, and therefore that it is only one's own actions whose meanings are likely to be self-

deceptively formulated. The act of formulating the motives of others, however, can easily be seen to be of a quite different kind, when it is remembered that far from being unconscious of the true deceptive nature of other individuals' actions, it will clearly be in our interests to have as profound an insight into their true nature as possible.

What this means then, is that whilst our motives for formulating the motives of others might be self-deceptively formulated - for instance as scientifically rather than personally relevant - the content of the motives we ascribe to others need not necessarily be inaccurate. On the contrary, the more closely our interpretation of the actions of others corresponds to their own unconscious motives, the better placed we are to prevent our own exploitation. Of course this does not in itself explain Freud or Darwin, or why psychoanalysis and evolution were not discovered twelve thousand years ago, but it does suggest that we have not evolved powerful barriers against the understanding of motivation in the abstract, as we have against understanding it in ourselves, and thus that it is possible to be correct in our abstract formulations about human motivation, even if this is not guaranteed.

This last observation does, however, raise two further issues which need explicitly to be mentioned. First there is the question of the therapeutic aim of psychoanalysis, which is to do precisely that which I am suggesting is impossible, and enable individuals to subjectively accept the true basis of their repressed unconscious motivations. The second is the demonstrable fact that individuals on occasion deceive themselves not merely about their own motivation, but also that of others. This point,

which will prove to be of considerable importance, will be examined in the next chapter, but for the time being, let us consider the first.

The first point which needs to be made about psychoanalytic therapy, is that it is characterized by the operation of *resistances*, the removal of which is seen as the chief task of analysis, but one which is notoriously difficult to achieve. Yet even given the assumption of such resistances, there does not seem to be any compelling reason why self-insight should ever be achieved. To see how something resembling self-insight might come about, it is necessary to consider whether there are any circumstances under which it would be adaptive to attain anything approaching a full awareness of one's own motives. Since, according to the view I am advancing, the chief reason for self-deception is the performance of self-serving actions for which publicly acceptable motives can be formulated, it would seem that the only circumstances under which such self-deceptions can become redundant are when the publicly acceptable motive which is formulated fails in its purpose. That is, when those who one is attempting to deceive, become aware that such a deception is being practised. However, it must be emphasised that this is very unlikely to be straightforward, since whilst there is a possibility that the deception may be restored, an awareness of the true nature of the motive would be disadvantageous. At the same time, a total denial of the basis of the accuser's insight, whilst possibly adaptive for a while, could not be expected to be plausible in the light of overwhelming evidence to the contrary.

What all of this suggests, therefore, is that providing the motive of which one is accused is more publicly acceptable than the true motive, even if closer to this than that which is consciously formulated, it may be adaptive to accept the motive of which one is accused. This is because, although not ideal from a deceptive point of view, it is at least believed, and because of the distance which still exists between it and the true motive, a degree of deception - and self-deception - may still be successfully practised. The implications of this for psychotherapy are therefore rather ironic, in that it follows from this that interpretations of one's actions which are in fact *farthest* from the truth, and so most publicly acceptable, are those which are most likely to be accepted. Those which, by contrast, are closest to the repressed reality, can be expected to be the most difficult to subjectively formulate, despite the intervention of the therapist.

If we consider this point in terms of the functions of repression I have been advancing, it is possible to see that in the case of one's actions being interpreted as revealing a deeper motive which is still publicly acceptable, the action implied by this motive, providing it is still acceptably close to the repressed, could be substituted for that which has been analysed. The adaptive significance of this is obvious. If a piece of deception fails, the most adaptive strategy will be to reformulate the deception, not as the truth, but as another, but more credible, deception. In terms of *self*-deception, therefore, once it is no longer adaptive to maintain one self-deception - because it has been correctly revealed as just that - it will be adaptive to replace it with another self-deception which has been ratified as both publicly acceptable and credible. However, in the event of the true motivation of an action being formulated by another individual, to self-

consciously formulate it as one's true motivation would negate the very purpose of the self-conscious formulation of one's motives. In other words, true insight into one's own motivation would seem to be ruled out by the very evolution of self-consciousness.

Does this therefore mean that all psychoanalytic interpretations which are accepted by the analysand are necessarily greater or lesser distortions of the truth? If this is the conclusion which must be reached, then not merely is doubt cast on the viability of psychoanalysis as a therapeutic method, but also on the adequacy of those theoretical assumptions derived from clinical experience. This conclusion clearly hinges upon the relationship which is assumed between becoming self-conscious of a wish to perform a certain action, and the performance of that action. If, however, we accept the argument that self-consciousness evolved not merely to facilitate the formulation of self-deceptive motives for its own sake, but instead so that only those actions for which publicly acceptable motives *can* be formulated are peformed, it is possible to see that the relationship between self-consciousness and action is not necessarily as straightforward as it appears. This is because whilst the performance of an action implies that its motives can be deceptively formulated, the formulation of motives does not necessarily imply that the action will be performed. Indeed, if consciousness represents the likely response of others to an action, the very fact that a wish is consciously formulated by the actor *as unacceptable,* suggests that it will not be enacted in that form. It would only be those actions, therefore, which are formulated as desirable, which are likely to be performed, and it is precisely the formulation of true motives by the actor, *but without their subsequent enactment,* which is the aim of

psychoanalysis, at least the mature method of classical analysis employed by Freud after 1920 (see Badcock 1988, ch.6).

This does not mean, of course, that when a repressed wish is self-consciously formulated it should cease to produce transformed representatives. Insofar as this assumption is made by psychoanalysis, it seems to be without biological foundation, and a product of a therapeutic bias - not an accusation easily made of Freud himself - which unjustifiably equates the return of repressed derivatives with mental abnormality.

In terms of the view offered here, then, we can now see that the assumption of the adaptive functions of repression I have outlined means that it is theoretically possible to gain true insight into one's motives, but only at the expense of their enactment in that form. Indeed, it suggests that in the event of another individual correctly intuiting the basis of one's true motivation, if the deceit cannot plausibly be maintained (the processes of which are discussed in subsequent chapters), it may be adaptive to self-consciously formulate one's true motivation as undesirable, so as to best advertise a commitment to its prevention.

I have tried in this chapter to outline a view of self-deception and its role in motivation which is compatible with both the insights of classical psychoanalytic theory, and modern evolutionary biology. In the next chapter, I wish to broaden this discussion to consider the possibility of *collective* motivation and self-deception, in relation both to the arguments advanced in this chapter, and those elaborated previously.

CHAPTER SEVEN

Inclusive Fitness and the Nature of the Group

The central argument of the last chapter was that the evolution of human consciousness can only be understood in terms of its deceptive functions; that is, in terms of the role of *repression* in facilitating the performance of actions which are both deceptive, but for which publicly acceptable motives can be formulated. As I pointed out, this argument suggests that whilst an accurate insight into one's own motives is unlikely to evolve, the ability to correctly intuit the deceptive motives of others could, correspondingly, be expected to evolve as a counter-measure. I also pointed out however, that this argument is complicated by the fact that self-deception appears not to be in reality as individualistic as this. Indeed, it is perfectly self-evident that actors do not merely attribute publicly acceptable motives to their own actions, but also to those of others. At first sight this realization appears to cast considerable doubt on the validity of my analysis: if individuals are as likely to attribute publicly acceptable motives to others as they are to themselves, then surely it cannot be argued that such self-deceptive formulations of motive have an adaptive value. In fact, following my argument, this realization would seem to have the effect of forcing exactly the opposite conclusion, since to deceive oneself about the motives of others must carry with it an adaptive penalty.

To see why this is not the conclusion which must necessarily be drawn, and to see how this problem relates to the arguments advanced in previous chapters, it is necessary to begin with a more detailed analysis of

the source of the contents of self-deception, or in psychoanalytic terms, the basis on which material is repressed and admitted to consciousness.

The Ontogenesis of Self-Deception

Throughout my discussions of psychoanalysis, and the relevance of psychoanalytic ideas to the problems posed by evolutionary biology, I have stressed the emphasis placed by Freud on phylogenetic considerations. At the same time, however, I have attempted to show how Freud's formulation avoids the trap of rigid biological determinism, and instead recognizes a complex interplay between phylogenesis and ontogenesis. My purpose here, before moving on to directly address the question of collective self-deception, is to elaborate somewhat on this relationship, and in particular to consider the evolutionary logic of the relationship on the matter of self-deception.

The thrust of my argument so far has been that since one's own genetic interests inevitably diverge from those of others, it will be adaptive to take the interests of others into account when acting, since a failure to do so could be expected to lead to costly conflicts, which could outweigh the advantages of genetically self-interested motivations. In order to explore exactly what it means to take the interests of others into account, however, I wish now to consider a number of possible alternatives, and stress the relative roles of ontogeny and phylogeny in each.

The first possibility is that we are simply selected not to wish to act in ways which are likely to enhance our own fitness at the expense of others.

Although such a position might appear to contravene the impossibility of the evolution of altruism, it is in fact theoretically possible since the cost of cheating could come to outweigh its advantages, if its detection and punishment was strongly enough selected. Yet, for the reasons given in the last chapter, although possible, such a situation seems highly unlikely to become an *evolutionarily stable strategy* (Maynard Smith 1974), since cheating in a more subtle way would always carry with it a much greater selective advantage, with the same immediate effect of avoiding the maladaptive consequences of detection (Trivers 1981, 1985).

The second possibility is that self-interested motives exist, but that the content of those which are denied conscious expression and enactment is a biological given. In other words, those impulses which undergo repression do so because biologically compelled. Although this formulation has the merit of allowing an independent source of self-interested motivation, it suffers from a degree of inflexibility, and for practical purposes has similar drawbacks to the first possibility. This is because were the content of that which is denied to consciousness determined entirely by biology, there would exist the possibilities either that fitness sacrifices are made when not necessary, or alternatively that inadequate sacrifices are made when greater sacrifices are adaptively useful. That is, such a state of affairs would not be able to take into account, and hence maximize the fitness gain from, any fluctuations in the degree of tolerance or awareness of other actors.

A third possibility, therefore, is that the content of the motives which are excluded from consciousness is determined entirely by the exigencies of

the immediate social environment. This would have the advantage of permitting a high degree of sensitivity to such fluctuations, but would also carry with it significant penalties. In particular, and this is the essence of the criticisms made of Marcuse's theory in chapter five, it would involve an equation of self-restraint with the unbridled genetic interests of others, which is tantamount to a genetic predisposition towards allowing oneself to be the victim of deception. In other words, it is equivalent to a genetic predisposition towards altruism.

The final possibility, however, is that whilst the content of that which is denied conscious expression is sensitive to fluctuations in what is publicly acceptable, that which is capable of being accepted as falling within the sphere of "public acceptability" has a biologically defined limit. In other words, restrictions placed upon the actor which go beyond those which it is to his genetic advantage to observe - that is, those which benefit other individuals at his expense - ought not to be accepted as the basis for repression. At the same time, if self- interested actions can be performed without incurring sanctions from those whose interests they work against, then it would clearly be in any actor's interests to take advantage of this. What this therefore suggests is the evolution of a degree of ontogenetic flexibility in the content of those self-serving motivations which are denied access to consciousness (and hence direct enactment), but an essential underlying biological blueprint, working to prevent self-deception from serving conflicting genetic interests.

It is in the light of this observation of the logic of self-deception, that the relationship which Freud posited between phylogeny and ontogeny in the

emergence of the ego and super-ego needs to be understood. I pointed out in chapter five that Freud's view of the super-ego was most certainly not entirely environmentally contingent; on the contrary, it is understood fundamentally as a phylogenetic acquisition dependent upon the operation of *primal repression*. At the same time, the ontogenetic emergence of particular super-egos is the consequence of the internalization of certain parental restrictions in early childhood:

> The installation of the super-ego can be described as a successful instance of identification with the parental agency (1939, S.E. 23, pp.63-64).

This dual origin - phylogenetic blueprint and ontogenetic detail - correspond, therefore, at least in outline, to the dual need for sensitivity to variations in public acceptability and the prevention of unbridled deception.

It is particularly significant for the present argument that Freud regarded the source of super-ego ontogenesis as identification with *parental* agencies. This is because, if indeed it is correct to regard the agency of the super-ego as performing the adaptive functions I am suggesting, the parental view of that which is publicly acceptable can be expected to be the most reliable available. This is in spite of the fact that, as Trivers has shown (1974, 1985), parent-offspring conflict can be expected to be a ubiquitous feature of parent-infant relations. The reason for this is that although, as Trivers demonstrates, the genetic interests of parents and their offspring are not identical, parental interests do nevertheless remain the closest to those of the infant that can be expected under normal

circumstances. Furthermore, if the strictly Freudian view of the matter I have so far advocated is the one which is accepted, the difference in interest which does exist between parents and their offspring is accommodated by the fundamentally conflictual model which is thereby assumed. This point is indeed recognized explicitly by Trivers:

> Since the personality and conscience of a child is expected to affect the child's altruism and selfishness, these characteristics may be a matter of disagreement between parent and offspring...So far as we know, personality and conscience are formed early in socialization, probably during the first five years of a child's life. We expect the child to develop during this time internal representations of its parents' viewpoints as well as its own. These may be in conflict, requiring mediation by some third entity. This suggests a similarity to Freud's system of the id, the super-ego, and the ego. The *id* represents internal, innate, egoistic impulses in the Freudian system. We might say it represents the offspring's own self- interest. The *super-ego* represents the internalized demands of the parents and is developed in interactions with them. The *ego*, in turn, acts as a referee, reconciling the demands of the id and super-ego (Trivers 1985, p.163, *italics in original*).

To identify with unrelated individuals on the other hand, and thus effectively adopt their standard of the publicly acceptable would, by virtue of the absence of any genetic similarity, again open the possibility of exploitation. Given the phylogenetic blueprint for the content of the super-ego, such a possibility would clearly not be without limit, but could nevertheless be expected to be real, and evolutionarily significant.

What this argument therefore suggests, is that the type of compromises which individuals actually make between fundamental fitness maximizing wishes, and the demands of public acceptability as mediated through parental authority are, to a degree, variable. But, where the expectations of evolutionary theory are borne out by classical psychoanalysis is in the assumption of a phylogenetic limit to this variability; in short, only that which it is adaptive to vary is in fact varied. This means then that the degree of self-deception which an individual actually practises - and hence according to the argument being developed here, the actions which are actually performed by way of a compromise - will exhibit some variation according to limits which are ontogenetically set to public acceptability in childhood.

Ontogenesis and Culture as Collective Self-Deception

As I remarked at the beginning of this chapter, a tendency to deceive oneself about the motives of others seems to be most unlikely to evolve, and yet it is quite clear that such a tendency does exist. Indeed, when the relationship I am suggesting between behaviour and self-deception is considered, this tendency would seem to come very close to being the rule rather than the exception. To see why such a realization is so problematic for the theory I am advancing, we need briefly to consider the evolutionary logic of the consequences of behavioural similarity.

My argument so far has been that behaviour is the product of compromise between an unconscious fitness maximizing wish which, if enacted, would benefit the actor at the expense of others (with ultimately

maladaptive consequences), and the internalized representative of certain parental prohibitions. The effect of this is that actions which are performed can be expected to be *deceptions*, for which publicly acceptable motives can be formulated, since it is only in this way that action can be both motivated according to the basic principles of evolutionary biology and yet still be successfully carried out in an environment of competing genetic interests. If, however, actions are indeed deceptive - that is, tend to benefit the actor at the cost of others - then it is difficult to see how not merely has there evolved no antipathy towards behaviourally similar others, but moreover why it is that human societies seem, on the contrary, to be *based* primarily upon sympathy towards behavioural similarity.

The reason for this difficulty is that if one's own actions are deceptive, then to welcome the very same deception in others - indeed to use the exhibition of such a deception as the basis for altruism - is equivalent to a motivated self-sacrifice. To illustrate this we could take the hypothetical example of a possible transformation of an unconscious desire to engage in *gross cheating*. Let us suppose that this unconscious desire meets with an internalized prohibition of such wishes (from the *super-ego*), and that the well known Freudian defence of *projection* is employed to deal with this wish as it seeks to return from the repressed.[1] Briefly, projection is the defence in which an unconscious wish is prevented from gaining conscious expression by consciously and compulsively attributing it to another individual. Thus, for example, the actor in question may habitually make accusations of non-reciprocation of other individuals,

[1] The mechanism of projection is discussed in detail, in connection with sexual object choice, in chapter nine.

and remain quite unconscious of the fact that the basis of this is his own desire to do the same. The adaptive significance of such a strategy is not difficult to see: by making accusations of this type, the actor affirms to any individual with whom he *is* prepared to engage in reciprocal interactions that this individual is trusted to reciprocate. At the same time, the awareness that the actor in question is acutely sensitive to attempts at cheating, could be expected to reduce the confidence of the actor with whom he is interacting in the possible success of any attempt at non-reciprocation. The effect of this, therefore, will be that the original actor actually gains from the interaction, by effectively reducing the chances of non-reciprocation (see Nesse & Lloyd 1990). In this way the strategy of projecting such a fitness enhancing desire, whilst apparently equivalent to its abandonment, may in fact result in the achievement of a more secure benefit by means of a more round-about route.

If we now apply this example to my argument, we can see that an individual who behaves in the way described could be expected to be selected to avoid interactions with others who behave in a similar way. This is because such behaviour confers a fitness advantage to its bearer at the expense of those with whom he interacts, and so to interact with an individual who behaves in the same way ought to result at best in perfect reciprocation, and at worst in a relative fitness loss. Yet even if perfect reciprocation were the likely outcome, interactions involving full-reciprocation ought to be systematically rejected in favour of those which confer an adaptive advantage, that is, those in which cheating prospers. In reality however, rather than exhibiting the sort of mild negative attraction which this argument predicts, it is obvious that similarly disposed

individuals attract. A particularly striking example of this, and pertinent to the example given here, is the sympathy which develops amongst individuals who share common negative evaluations of some other individual or group, and in particular, one might consider the formation of groups based upon racial hatred. If the irrational accusations and attribution of negative characteristics which is typical of such groups is indeed based upon a projection of the unconscious wishes of the accusers, then, on this argument, such accusers ought to be selected to avoid interactions with each other. What typically happens instead, however, is that individuals who adopt similar compromises (*defences* in psychoanalytic terms), form into groups which collectively pursue the actions which constitute the compromise. In other words, individuals who practise similar self-deceptions, seem to deceive themselves about the motives of each other, and therefore, on this basis, effectively render each other altruism. As I have remarked, this could be interpreted as a pattern of pure reciprocity, but it should be remembered that it is also effectively a self-imposed restriction on deception, and so is *relatively* disadvantageous; certainly it must confer sufficient a disadvantage to confound the explanation of its evident strength.

The problem can however be solved if we briefly return to the central insight of Hamilton's model (1964a,b) which was discussed in chapter one. The essence of this insight, it will be recalled, is that since selection can only be considered to operate at the level of the individual gene, calculations of genetic self-interest must also take into account *kinship*; that is, fitness becomes *inclusive fitness*. In this way, as I mentioned in the last chapter, it is argued that one of the fundamental forms of apparent

altruism is *kin* altruism; that is, apparent acts of altruism are explained by the probability that an identical copy of a gene will appear in the recipient by common descent. But the question remains, how could this phenomenon explain the case of apparent altruism which I am currently discussing? After all, what we are dealing with in this case is not genetic similarity at all, but *behavioural* similarity which, I have strenuously argued, is not rigidly biologically determined as are purely physical characteristics.

To answer this question we need to consider the relationship which I have so far implicitly posited between behavioural and genetic similarity, which at first sight may appear unrelated, but which in fact, as further consideration reveals, are nothing of the kind. I ponted out earlier in this chapter that a crucial point at which evolutionary expectations are borne out by psychoanalytic theory is in the ontogenetic emergence of the specific contents of the individual super-ego (within a basic phylogenetic blueprint), which facilitates sensitivity to fluctuations in standards of public acceptability. Given in addition, however, the convergence of evolutionary and psychoanalytic theory on the matter of the parental source of super-ego ontogenesis, and the relationship which exists between this and actual behaviour enacted by way of compromises, it follows that similar behaviour is the product of similar internalized prohibitions and therefore a reliable indicator of relatedness. In other words, since similar behaviour results from similar parental prohibitions, it is reasonable to surmise that under the circumstances of our environment of evolutionary adaptedness - primal hunter-gatherer

societies - similar behaviour, more often than not, would indicate similar parenthood.

Considered from this point of view, the apparent altruism which would be involved in collective self-deceptions based upon behvioural similarity could be explained as an example of kin-altruism. Because the genetic interests of kin overlap, so do benefits derived from self-deception, even though this involves technically becoming the *victim* of a relative's deception. This is because one's victimization confers a fitness benefit to another who possesses a proportion of one's own genes, and so those genes enjoy a net benefit through the advantage conferred on the relative.

It is a striking fact that Freud advanced a model of the processes involved in group formation which adumbrates, in the closest detail, the modern evolutionary calculations which underlie this reasoning. This point has been developed by Christopher Badcock, who, in particular, has pointed out the similarity between the psychoanalytic concept of *identification* and the evolutionary expectation of a psychological mechanism mediating kin-altruism (see also Rancour-Laferriere 1981):

> Freud's declaration that 'Identification is known to psychoanalysis as the earliest expression of an emotional tie with another person' [1921, S.E.18, p.105] points towards the considerable psychological evidence underpinning the contention that identification is basic to kin altruism in human beings. The element of altruism comes about, evidently, by making individuals identify with others to the extent of doing for them what they might otherwise only do for themselves. Yet one of Freud's most important insights was that this ability to

> overcome one's sense of difference from - and indifference to - the other is a consequence, not of an innate tendency to form indiscriminate identifications with other people, but of the dynamic structure of the process of identification itself (Badcock 1986, pp101-102).

It is precisely the dynamic structure of the process of identification, as conceived by Freud, which gives the concept of identification such pertinence to the present discussion. It is in his major work on collective psychology, Group Psychology and the Analysis of the Ego (1921, S.E 18), that Freud gives his most thorough discussion of the phenomenon of identification. Here he stresses that identification in adulthood is based upon the infantile prototypes (that is, with the parents) which formed the basis of super-ego ontogenesis. Identifications, however, and this is where Freud's analysis is crucial for the present argument, do not come about directly between individuals but rather occur indirectly as a result of the perception of a common relationship with an *ideal*. Specifically, Freud argued:

> *A primary group of this kind is a number of individuals who have put one and the same object in place of their ego ideal* [super-ego] *and have consequently identified with one another in their ego* (1921, S.E. 18, p.116, *italics in original*).

Exactly as predicted by the argument above, Freud's model of identification is based upon the surest possible predictor of relatedness: a common parent, as indicated by a common ego-ideal. Where the convergence of psychoanalytic and evolutionary theory is perhaps most important, however, is in their recognition of an individual, instinctual

basis to apparently arbitrary collective phenomena. For, not only does the concept of identification provide an explanation for the apparent altruism involved in group phenomena, but also suggests that the *content* of group behaviour - in a word, *culture* - can be explained in exactly the same way as individual behaviour. In short, just as individual behaviour is the result of a compromise between a fitness maximizing wish and the demands of public acceptability, so it is for culture, with the difference that here a number of individuals have internalized similar standards of public acceptability, and hence are practising similar self-deceptions and resulting behavioural compromises.

This argument suggests moreover, that the penalties associated with rendering altruism to unrelated individuals should exert a selective pressure on both the emphasis of group identity, and the detection of difference. Thus, individuals sharing behavioural compromises similar enough to indicate relatedness, could be expected to be selected to emphasise their similarity against their differences, and at the same time to be able to discriminate against those not similar enough to indicate relatedness. This suggests first of all that "cultural" groups of all kinds should be defined by a common behavioural pattern which has strictly defined limits. But because of the origin of the behavioural similarity involved, genuine relatedness would only be indicated by a motivated commitment to the behavioural pattern in question; in other words, a genuine compromise. For this reason, anyone traversing the limits of the group's behavioural pattern could be reliably predicted to have formed a behavioural compromise based upon a different internalized standard of acceptability; that is, they would not be related. At the same time, this

suggests that because altruism, either direct, or in the form of the sharing of self-deceptions, is a consequence of behavioural similarity, an obvious form of cheating would be *mimicry*, that is, exaggerating one's commitment to the compromises of the group whilst in fact maintaining commitment to alternative compromises. Like all forms of cheating, however, we can expect this to have its counterpart in the evolution of the means for its detection. For instance, in the case of racial hatred mentioned earlier, we could consider the likely fate of an individual who professed a genuine disregard for the particular despised group but who was found secretly displaying considerable friendship towards one of its members. A better example still would be a professed member of a religious organization which strictly prohibits extra-marital relationships being caught *in flagrante*; what is at issue in this case is not the desire to engage in such behaviour - we can assume that this is the latent meaning behind the abstemious behaviour of all members - but the fact that the individual in question adopted a different compromise, and therefore displayed a different standard of public acceptability. In this case, the action performed indicates a weaker standard of public acceptability, and so not merely is the individual in question displaying a different source of internalized restraint - and thus is not related - but is actually enjoying a fitness gain from the action, logically at the expense of those in whom such behaviour is prohibited. It thus follows that such group members should be selected to become fully aware of the selfish nature of this conduct, and so cease any identification with this individual. In this way, the suggestion that "cultural" groups of all kinds are formed on the basis of collective self-deceptions based on kinship as indicated by a common source of internalized prohibition, may throw considerable light upon the

general question of normative commitment, and indeed on the consequences of normative deviation.

It has been often enough remarked too that not merely do the behaviourally similar attract, but that they do so in opposition to the dissimilar. Freud, for example, notes that:

> Of two neighbouring towns each is the other's most jealous rival; every little canton looks down upon the others with contempt. Closely related races keep one another at arm's length; the South German cannot endure the North German, the Englishman casts every type of aspersion upon the Scot, the Spaniard despises the Portuguese. We are no longer astonished that greater differences should lead to an almost insuperable repugnance, such as the Gallic people feel for the German, the Aryan for the semite, and the white races for the coloured (1921, S.E. 18. p. 101).

Similarly, Serge Moscovici observes:

> The active and passionate fellow-feeling experienced for one's compatriots or clansmen or for those sharing the same idea had as its counterpart a no less active and passionate antipathy for the nationals of other countries, the inhabitants of a neighbouring town, those with a different religion or for foreigners, blacks, Jews and the rest.
>
> The combination of fellow-feeling for 'us' and antipathy for 'them' had the corollary of a feeling that 'we' were superior. Men are frequently prepared to recognise as fully human only those individuals belonging to their own ethnic, linguistic or national group and to treat outsiders as less than human. The names of many Amerindian tribes simply mean 'men',

> 'flesh' or 'people' (the Navajos, Apaches and Utes, for example) and the Greeks were kind enough to call all foreigners barbarians (Moscovici, 1985, p.247).

If similarity does indeed have the significance I am claiming, then observations such as these are readily explicable as the counterpart to kin-altruism: an *absence* of altruism in dealings with unrelated individuals, and a correspondingly acute awareness of any possible deceptions which such individuals may be practising. Of course this analysis of collective self-deception as based upon kinship is a striking example of the effect of evolutionary time lags which was outlined in earlier chapters. Quite clearly, the altruism which is rendered in such situations is, under modern circumstances, maladaptive in the sense that it is not, in general, directed towards kin at all, but unrelated individuals. Nevertheless, this merely points to the fact that under the conditions of our evolutionary adaptation, such behavioural similarity *would* have been a reliable indicator of relatedness.

Considered with the criticisms I have already made of conventional social theory in mind, the approach outlined here suggests that not only can apparently maladaptive aspects of *individual* behaviour be explained from an evolutionary point of view, but so also can its collective and apparently normative aspects. Unlike conventional sociological perspectives which regard the norm as essentially arbitrary and uni-directionally imposed upon individual voluntarism to meet the demands of the social system, the economic infrasturucture, or whatever other abstract *sui generis* entity, this approach sees the norm as a collective behavioural compromise between a biologically determined fitness maximising wish

and the exigencies of public acceptability. The collective nature of apparent normative conformity, which has traditionally been interpreted as *socialization* or *enculturation*, is here understood as a manifestation kin-altruism brought about by collective self-deception. In this way, the theoretically unacceptable assumptions which underlie conventional approaches to normative culture are avoided. Collective modes of behaviour are not seen as possessing an arbitrarily imposed content, and hence as externally motivated; rather, they possess a content which is determined by biologically motivated transformations of fitness maximising wishes. Similarly, variation in these collective modes of behaviour, which as we have seen, seem to pose such a problem for conventional sociobiological explanations, is entirely expectable from this perspective, since they reflect varying sources of internal prohibition and resulting compromises.

It is clear that the analysis offered here lends considerable support also to Freud's contention that there are parallels between individual psychopathology and culture, and indeed that the latter is little more than a collective expression of the former.[2] For example, in a paper dealing with the parallels between religious practices and obsessional neurosis he writes:

> It is easy to see where the resemblances lie between neurotic ceremonials and the sacred acts of religious ritual: in the qualms of

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[2] For an attempt to develop the parallels between the two amongst Australian Aborigines, see for example, Roheim (1934); for an account of the parallels in the case of the history of religion, see Badcock (1980).

> conscience brought on by their neglect, in their complete isolation from all other actions (shown in the prohibition against interruption) and in the conscientiousness with which they are carried out in every detail (1907, S.E. 9, p.119).

A further parallel is indicated in the unconscious content and motivation of neurosis and religious practices:

> It is one of the conditions of the illness that the person who is obeying a compulsion carries it out without understanding its meaning - or at any rate its chief meaning. It is only thanks to the efforts of psychoanalytic treatment that he becomes conscious of the meaning of his obsessive action and, with it, of the motives that are impelling him to it. We express this important fact by saying that the obsessive action serves to express *unconscious* motives and ideas. In this we seem to find a further departure from religious practices; but we must remember that as a rule the ordinary pious individual, too, performs a ceremonial without concerning himself with its significance, although priests and scientific investigators may be familiar with the - mostly symbolic - meaning of the ritual. In all believers, however, the motives which impel them to religious practices are unknown to them are or represented in consciousness by others which are advanced in their place (ibid., pp. 122-123).

In other words, collective cultural phenomena - such as religion - are the product of self-deceptive compromises between internalized prohibitions and unconscious repressed wishes, in exactly the same way as individual behaviour, and as with individuial behaviour, their motivation can only be understood in terms of biologically determined latent meaning. It is precisely by the reduction of the motivation of *all* behaviour - individual or collective, normal or pathological - to such latent meanings that

psychoanalysis is able to break the hopeless circularity of conventional social scientific explanation, and provide an evolutionarily satisfactory model of the motivation of even such behaviour as appears to defy evolutionary explanation absolutely.

I have in this chapter tried to show how the apparent paradox of collective self-deception can be resolved when the biology of kinship is considered. In so doing, however, I have suggested that, as psychoanalytic theory would claim, the manifest meaning and content of collectively undertaken self-deceptions is determined in exactly the same way as those undertaken individually; that is they are motivated transformations of fitness maximising wishes, frustrated by the necessities of the genetic self-interest of others, as internalized in childhood on the basis of certain phylogenetically determined limits and patterns. In the chapters which follow, I wish to concentrate upon the exemplification of the mechanisms by which such fitness maximising wishes are transformed, and how such an approach is both empirically and theoretically more compelling than either the sociobiological or the sociological alternatives. I have chosen to illustrate these points with the example of sexual behaviour, since whilst this is a sphere of behaviour which, in view of the evolutionary arguments I have advanced, ought to be most heavily determined by biology, in fact displays considerable variation and evidence of maladaptiveness which, some would argue, justifies its removal from the biological realm altogether.

CHAPTER EIGHT

Sexual Variation 1: Evolution, Commitment and Deception.

The Problem of Sexual Variation

There are a number of reasons why sexual behaviour is an appropriate example to illustrate the problems posed by the evolutionary analysis of human behaviour. By far the most obvious of these is the fact that of all possible facets of behaviour it is the one which should, it seems, in view of the arguments so far presented, be most clearly and unambiguously under the control of biological mechanisms. Not only is sexual object choice crucial for the evolutionary success of all other genetic adaptations, but so also is sexual *behaviour* towards the chosen object. Simple evolutionary theory suggests, therefore, that this should strongly favour the selection of the optimization of both object choice and sexual behaviour, with a common optimal pattern quickly developing. Gross non-reproductive deviations from this pattern, by contrast, should be eliminated as soon as they emerge, with weaker deviations gradually eliminated by increasingly successful patterns. The reality of the situation is, however, not so straightforward, since not merely do we find a startling array of objects of human sexual desire, but also of behaviours adopted towards these objects. Leaving aside for a moment the issue of "minority" sexual object choices - such as those which cross sexual, species and even animate/inanimate boundaries - there seem to be huge "cultural" variations in criteria of sexual desirability even amongst objects which meet the basic gross reproductive criteria which evolutionary theory predicts: Ford & Beach,

for example, in their classic review of some 190 tribal societies conclude that there are "few if any universal standards of sexual attractiveness"(Ford & Beach 1951, p.86).

It has, however, been suggested by those sympathetic to evolutionary biology that despite an admittedly high degree of variation in definitions of sexual attractiveness, there are certain abstract universals which underlie this variation. In particular, Ian Vine has suggested , following Donald Symons (1979), that it is possible to isolate two substantive universals of female beauty: indicators of general health and of youthfulness, both of which are of obvious relevance to reproductive success (Vine 1989). Moreover, even the evidence of Ford & Beach (1951) suggests that there are universal features in the female assessment of male attractiveness which differ from those in the case of male assessment of females:

> One very interesting generalization is that in most societies the physical beauty of the female receives more explicit attention than does the handsomeness of the man. The attractiveness of the man usually depends predominantly upon his skills and prowess rather than upon his physical appearance (Ford & Beach 1951, p.86).

Again, it could plausibly be argued that "skills and prowess" are of obvious relevance to the male's ultimate reproductive success, particularly in a species where co-operative hunting by males evidently played such a crucial role in its evolution (Hill 1982). The problem which such an argument faces, however, is the familiar one that generalizations of this sort are quite impotent when faced with empirically significant variation

(see chapter 2 above). Moreover, in this particular case, exceptions to the generalization which is alleged to obtain can be found with such ease and frequency that it can hardly be considered meaningful at all.

This is not of course to deny that one should expect standards of attractiveness to be relatively *abstract*. On the contrary, as Ian Vine has emphasised, any biologically determined standard by which the desirability of objects is assessed needs to be relative to the potential mates likely to be available to any actor (Vine 1989, p.13). It is quite clear that an actor biologically predisposed to become sexually aroused only by such objects as in whom highly precise conditions are met, would be at a distinct selective disadvantage over even a rather indiscriminate actor in the not unlikely circumstances that someone meeting these criteria was either unavailable or, if available, unwilling. Whilst this does indeed suggest that excessive discrimination ought, in principle, to be selected against, what it does not explain is the considerable variation which exists in sexual *ideals*, indeed the existence of sexual ideals at all. This is because if standards of attractiveness need merely to be abstract enough to allow for limitations in the available objects, there does not, at aleast at first sight, seem to be any reason why apparently detailed preferences *do* develop.

Perhaps this can seen be most clearly by considering not only rather elusive *cross-cultural* variations, but the altogether more obvious and pronounced variations which occur intra-culturally. For example, we could take *fetishism*, which has been defined as:

> ...a form of behaviour wherein sexual activity or sexual fantasy focuses to an unusual extent upon a body part or an inanimate object rather than on a person as a whole (Gosselin & Wilson, 1984, p.90).

Evolutionary biologists have, of course, recognized the difficulty which such instances pose, and have attempted to explain them in terms of evolutionary theory. In particular, it has been suggested by some (e.g Daly & Wilson 1983; Symons 1979) that since sexual "deviations" (with the possible, although perhaps partial exception of homosexuality) are almost entirely male preserves, they are explicable with reference to the sex-specificity of human reproductive costs. Essentially this view holds that because the cost of the sexual act - in terms of subsequent investment - is far greater for the female than for the male, the female would be expected to evolve highly discriminating strategies for sexual object choice, whilst the male could be expected to be relatively indiscriminate. Thus, the phenomenon in question might be explicable simply as the consequence of an evolved lack of discrimination on the part of the male, reflecting the relative "inexpensiveness" of his participation.

Whilst, as I shall later show, this line of reasoning has some relevance, it is unacceptable as an explanation of sexual variation. First of all, the sex-specificity of sexual variation is by no means an conclusive as supporters of this theory assume, and despite a general lack of reported cases (e.g. North 1970), examples of female variations do exist (see Caprio 1957, pp. 156-157, for case-histories of lesbianism associated with other practices). Secondly, and this is the chief objection, an explanation of sexual variation relying on an evolved tendency towards sexual indiscrimination, completely fails to take account of the *compulsive* and frequently

exclusive character of much of this behaviour. Moreover, not only is behaviour such as fetishism compulsive and exclusive, it seems by its very nature to be precisely the opposite of indiscriminate: North's account of rubber-fetishism illustrates the highly detailed requirements of such fetishists:

> Features for which the fetishist demand is considerable are: high and tight collars, back-fastenings, straps, buckles and laces; very tight fittings in general, rubber linings to leather and P.V.C clothes. complete head and face coverings, corsets and waist "cinchers" (North 1970, pp.69-70).

Similarly a sado-masochistic and fetishistic informant of North's, Mr F.H.J., details his requirements for the ideal instrument of punishment:

> "I have very positive ideas on the type of cane to be used. The purpose of the exercise is not really to punish but to stimulate. Therefore the schoolmaster's twelve of the best accross a trousered or knickered bottom with a medium weight three-quarter foot cane leaving black or blue stripes is not the answer. The cane must be long, thin and very swishy and used very hard and always accross a bare bottom. It will then sting terribly while making only a thin, faint red stripe accross the bottom. A large number of strokes can then be given without being really brutal" (North 1970, p.186-7).

Not only are the requirements of fetishists extremely detailed, but, according to North (1970) willing participants are often extraordinarily difficult to find, with the effect that such individuals find sexual satisfaction abnormally difficult to obtain. This hardly supports the conclusion that the sexually deviant are indiscriminate, and when the

risks of arrest and social ostracism associated with many forms of sexual deviance are considered - for instance, exhibitionism, voyeurism, sado-masochism and frottage - it is suggested that they are driven by anything but indifference; rather, an irresistable compulsion to engage in the specific activity in question in favour of all others.

It is, however, worth pointing out that it is not merely evolutionary biology which faces difficulties in explaianing such behaviour, but so also do "cultural" theories. Leaving aside the biological objections to conventional social scientific explanations discussed in detail in chapter three, it is quite evident that for a theory which relies for its force upon the assumption of arbitrary, externally constituted systems of signification with the power to motivate action in accordance with these, the presence of action in which this culturally constituted significance is ignored is clearly difficult to explain. This point may be illustrated if we take again the example of fetishism. In fact reliable data on the range of fetish objects is difficult to obtain, probably because fetishists are generally secretive about their deviation, and rarely feel the need or desire to seek treatment. But the fact that such fetishes which have been documented include so wide a range of objects as safety-pins (Mitchell, Falconer & Hill, 1954), wet canvas shoes (Epstein 1975), and P.V.C. and rubber clothing (North 1970), to list but a few, suggests that, if anything, they are chosen *in spite* of any culturally determined significance.

In fact, the only sociological tradition to seriously attempt an explanation of human sexual deviation - symbolic interactionism - explicitly denies the sort of determinism typical of structural sociology; at the same time,

however, the notion of an external motivating culture is as dominant as ever. The Iowa interactionist, Kuhn writes:

> Sex acts, sexual objects, sexual partners (human or otherwise) like all other objects towards which human beings behave are social objects; that is they have meanings because meanings are assigned to them by groups of which human beings are members for there is nothing in the physiology of man which gives a dependable clue as to what pattern of activity will be followed toward them. The meanings of these social objects are mediated to the individual by means of language just as in the case of all other social objects. That the communicators which involve these definitions are frequently - at least in our society - surreptitious and characterized by a huge degree of innuendo does not in any wise diminish the truth of this assertion. In short the sexual motives which human beings have are derived from the social roles they play; like all other motives these would not have been possible were not the actions physiologically possible, but the physiology does not supply the motives, designate the partners, invest the objects with performed passion, nor even dictate the objectives to be achieved (Kuhn 1954, quoted in Plummer 1982, p.225).

I have no wish to repeat here my arguments against such a conception of motivation, which are expressed in chapter 3 above; instead I mean to emphasise the inadequacy of this formulation in coming to terms with the empirical reality of sexual behaviour. According to the symbolic interactionist point of view, and well illustrated by this quotation from Kuhn, sexual objects become sexual objects exclusively as a result of the imposition of sexual meaning by the social group. As Ken Plummer has expressed it: "sexuality is in a broad sense assembled from the cultural categories currently available" (Plummer 1982, p.235). In this way, it is

suggested that all that is of sociological interest are the categories through which individual sexualities are constructed, since without them there would be no sexuality of any sort; normality (as expected by biological theory) is no less a cultural construct than deviance. The implication of this, therefore, is that if actors did not grasp the relevant cultural categories defining sexualities and their consequent objects and activities of arousal, there would be no sexual activity. Similarly, were there an array of sexual categories entirely different from those which humanity has to date known, sexual activity would have been manifested quite differently, its only biological constraint being the "physiologically possible": a loose constraint indeed!

Leaving aside, as I have said, the blatant biological ignorance of such a view, the largest empirical challenge which this position faces is the possibility of the emergence of a sexual practice or orientation not corresponding to a prevailing cultural category: in other words, the invention of perversion. Although it may be argued that the existence of *pre-categorical* sexual activity, as it were, is evidence of the innate fluidity of sexuality, and its openness to cultural construction, such a position would be hopelessly inconsistent, since it would involve the arbitrary attribution of biological determination to certain categories of behaviour and not others. In other words, it would involve a straight reversal of the crude biological distinction between normal and abnormal sexuality, with private and novel deviations being regarded as natural, whilst common publicly acknowledged activities such as heterosexual monogamy would be dismissed as an arbitrary social construction. The difficulty of course would be that this formulation would have no more grounds for making

the distinction than the one it seeks to criticize; in many ways, of course, it would have considerably less.

Although it is of course true that details of some sexual variations are publicly available - through medical and psychological text-books, pornographic literature etc. - many, in detail anyway, are not, either because they are novel inventions of the actor, or because they develop prior to any conceptual grasp of their nature or deviance. There is in fact ample evidence that the sort of pre-categorical sexual activity to which I am referring does indeed occur, which, in my view, removes any empirical credibility the interactionist view may have claimed. Taking again the example of fetishism, the following is an extract from another of North's informants who describes his childhood "invention" of an elaborate fetishistic and sado-masochistc perversion:

> "This was the time of direction of labour and Mother was forced to go out and take some job in a clothing warehouse office (typical democracy we live in). Anyway the house was empty when I used to return from school in the afternoon, and I used to go upstairs, lay the mac on the bed, strip naked, bind my legs and arms together, and fall face down onto the mac, this resulted in ejaculation generally without masturbation. This was superseded by wearing the mac, strapping my ankles, knees thighs together and passing a rope through a hook in the pantry ceiling, tying it to my ankles and heaving on the rope till I was hanging head down from the ceiling. I fastened the rope to a hook and used to put the mac hood on, fasten a tight collar round my neck, strap my hands together and then just 'think' and slightly 'strain'. This used to result in extreme erection and forced ejaculation against the mac's inside" (North 1970, pp.165-66).

It could hardly be said that the sexual significance of these practices derives from any pre-existing cultural categories.

A further objection which this line of argument suggests is that the approach in question cannot explain why a particular sexual variation should be constructed, indeed why any variation should be constructed at all. If, as this view holds, the compulsion to engage in a deviant sexual practice is consequent upon a process by which the actor defines himself as such a deviant, the question of the source of this definition is raised. If the definition must be actively applied by other actors, then cases where the deviant practice is engaged in- or acted out in fantasy - entirely without the knowledge of others (perhaps the majority) cannot be explained. If, however, it is argued that the definition is applied by the actor himself, on the basis of prevailing cultural categories, then the question of why they are applied in some cases and not others is left open. This is because the theory, in this form, assumes no pre-cultural variation in sexual object choice, and hence any actor with a grasp of these prevailing cultural categories would be theoretically as likely as any other to become a compulsive pervert. In other words, what once again becomes of central interest, this time for purely empirical reasons, is the source of the original motivation to perform the actions in question.

The Evolution of Sexual Commitment

It should be clear from the foregoing that the problem with which we are faced is essentially one of explaining the origin of variation in subjectively experienced sexual desire. In particular, the problem for evolutionary biology is the existence of subjective, and often compulsive, preferences for the maladaptive. In order to examine whether the method of analysis I have so far proposed can shed light on this particular problem, I propose to begin by considering the general issue of the evolutionary logic of subjective preference, or, *sexual commitment*. From there it will be possible to see whether the adaptive psychological mechanisms which are thereby suggested can, with a consideration of the effects of evolutionary time-lags, explain apparently inexplicable patterns of sexual commitment to adaptively less than optimum objects, in terms of evolutionary theory.

Perhaps the most obvious point to be made about commitment to a sexual object is that it will always be less than fully adaptive for the commitment to be total. The simplest reason for this is that such a situation would place the actor at a severe disadvantage in the event of that object's prolonged absence or death. Similarly, such total commitment would be maladaptive in the event of the possibility of an actor's inclusive fitness being enhanced by the availability of an adaptively superior object. For instance, a male who maintained total sexual commitment to a non-reproductive female would certainly be at a selective disadvantage over one who abandoned such a female in favour of another who was capable of reproducing. [1] To fully appreciate the consequences of sexual

[1]This example is rather contentious, since there is a debate as to whether the human female's menopause is in fact an adaptation or an artifact. Dawkins (1989) claims that it is an adaptation which has the effect of diverting the effort of the mature female into the assistance of her existing offspring; Symons (1979) claims that it is an artifact produced by modern circumstances, which allow females to live to an age which they would not have

commitment, however, it is necessary to consider the relative costs and benefits of male and female reproductive investment.

From a purely physiological point of view, as has already been mentioned, by virtue of internal gestation, the reproductive investment of the mammalian female is many times greater than that of the male. The logical consequence of this, therefore, is that males could potentially increase their reproductive success by many times by not restricting themselves to a single female, and indeed, this is born out by studies of the vast majority of mammalian species (see, for example, Eisenberg 1966; Le Boeuf, 1974, illustrates this with the rather extreme case of the elephant seal in which males produce up to twenty times as many offspring as females; see also Symons 1979, Ch,1). At first sight this argument appears to suggest the existence of strong pressures towards the selection of far higher levels of sexual commitment in the female than in the male. This is however misleading, principally because it fails to take account of consequences, other than impregnation, which copulation may have for females. In particular, it ignores the impact of male provisioning.

Considerable evidence exists to support the view that a crucial factor in the evolution of the specifically human mind was male cooperative hunting, and the consequent male provisioning of females and their offspring with meat (Symons 1979, Hill 1981, Badcock 1990; see also, Eisenberg 1966). Badcock (1990), following calculations by Hill (1981), suggests that because the nutritional value of meat, expressed in relation to the time necessary

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reached in the primal environment. In fact, there is no reason why the two positions should be incompatible.

to obtain and process it, is approximately ten times that of vegetation (p.148), strong selective pressures would have existed for (polygynously mated) females to prefer to copulate with successful male hunters, in return for donations of meat. As I shall later suggest, this observation has extremely important consequences for the evolution of sexual commitment, but for the present it is sufficient to note that once females have more to gain from copulations than insemination - i.e. donations of meat which can be used to provision *any* of their offspring - a strategy of total commitment can be seen to be theoretically maladaptive for the female as well as the male.

But if it is clear that total sexual commitment should be in principle a significantly less than optimum strategy for both males and females, it is equally clear that the same applies to its opposite: total indiscrimination, or *sexual promiscuity*. That this should be the case for the female is obvious, the reasons for which have already been discussed. In the case of the male, however, the situation is rather more complex, and has to do with the concealement of ovulation in the human female. Unlike female primates such as chimpanzees and gelada baboons who advertise their periods of fertility by means of elaborate physical and behavioural changes, which males of the species respond to with enormous sexual excitement, the human female's fertile period is virtually impossible to detect(Alexander & Noonan 1979; Burley 1979; Benshoof & Thornhill 1979; Symons 1979; Turke 1989; Badcock 1990). It has been hypothesised by a number of authors that the characteristically human loss of advertised ovulation is explicable as a strategy by which females secure extended periods of investment from males (Alexander & Noonan 1979; Hill 1981;

Badcock 1990). Relating this phenomenon to the role of male hunting and meat provisioning in hominid evolution, Badcock writes:

> If only fertilization is at stake, both male and female have an interest in making sure that copulations occur predominantly during estrus. But if provisioning females with highly nutritious food is at issue the interests of the sexes differ dramatically.
>
> From the point of view of a female, advertising estrus becomes a losing tactic if meat can only be had in exchange for copulations. This is because...males who will give her meat when she is not likely to conceive will find themselves at a reproductive disadvantage compared to those who reserve their gifts for her fertile period (Badcock 1990, p.157).

In this way, the gradual development of increasingly concealed ovulation, probably through increasing, rather than decreasing the period of the advertisement of ovulation (Turke 1989), would effectively force males into a situation in which to secure the fertilization of a particular female, a longer period of sustained copulation with that female - and hence, we must assume meat donation - becomes necessary.

There does, however, appear to be a possible weakness in this analysis. It could be argued that an alternative male strategy, which in net terms would produce identical results, could just as easily be envisaged. This is that precisely because the concealement of ovulation leads to a much reduced probability that any one copulation will lead to fertilisation, males would be selected who invested *less* in each female, whilst correspondingly increasing the number of females with whom they interact sexually. Certainly from the female point of view this state of

affairs would appear to make little difference: providing that each female is the recipient of many meat donations, their source - as long as from competent hunters, as would obviously be usual - is unimportant. Yet considered from the male point of view, this scenario has a major drawback. This is that the situations described would only produce equal results for any given male if it could be assumed that each copulation was potentially as valuable as any other in terms of inclusive fitness. Since, however, we can assume the existence of genetic variation in any population, it is quite clear that the offspring of some females will be fitter than others. The consequence of this, therefore, is that there will exist pressures for the selection of males who prefer to copulate with females who will best enhance their inclusive fitness. In consequence of concealed ovulation then, males will be selected to copulate for longer, in exchange for larger donations of meat, with the most adaptively viable females.

Even if we accept, however, that the fact of the concealement of ovulation would have necessitated longer periods of sexual commitment to individual females, the period of commitment in question remains very small. Moreover, the "commitment" required need by no means be exclusive, since as long as his meat resources could stand it, the male would be able to maintain sustained copluations with more than one female during the same period. It should be noted, however, that this argument assumes that the exchange of meat for copulations is taken literally; that is, a single piece of meat is exchanged for a single copulation. In reality such a situation is very unlikely to evolve. This is because differential parental investment would place the female at a major disadvantage in this exchange: a single donation of meat at the moment of

conception - even a number of such donations during perhaps one or two of the female's cycles - would make a hardly discernable difference to the gestating mother, and practically none at all to the subsequent offspring. In this way, sustained provisioning of meat to particular females will in fact be selected in males by the pressure of female choice. Whilst it could be replied, as I suggested earlier, that the source of the donations of meat is unimportant from the female's point of view, such an argument ignores the likely consequences of pregnancy and incumberance with young offspring for female attractiveness to successful hunters.

The reason why we should expect such factors to diminish female attractiveness is not difficult to see, once the adaptive benefit to the male of exchanging meat for copulations is fully considered. Fertilization alone, which I have so far implied is the chief male gain from the exchange, is in fact totally irrelevant to the male's inclusive fitness without the crucial additional factors of successful gestation, birth, maturation to adulthood and subsequent successful reproduction.[2] Thus, since meat has an adaptively enhancing effect for females, via the provisioning of their offspring, then so must it also for males, via exactly the same route. [3] The effect of this therefore, is that whilst it is clearly in the evolutionary interests of males to make continued meat donations to the mothers of their offspring, the costs associated with such a strategy make *confidence of paternity* a factor of crucial importance (Alexander & Noonan 1979; Turke

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[2]I ignore, of course, for the purposes of the present argument, the possibility of offspring contributing to parental inclusive fitness by supporting siblings.

[3]This also suggests that male attempts to circumvent female choice altogether by securing forced matings ought to be selected against, at least as a dominant strategy. It does not, however, seem to preclude the possibility that such a strategy could evolve as a "subordinate" strategy, when, for instance, the regular route for the male is closed.

1989). In other words, whilst a male can significantly enhance his inclusive fitness by provisioning his own offspring, provisioning those of other males - albeit inadvertently - carries with it fitness penalties of equal proportions. It therefore seems highly likely that selection should favour males who regard females with either born, or perhaps especially unborn offspring of other males, as significantly less attractive than others without such offspring. In this way, it seems likely that males will be selected to provision such females with decreasing frequency, the more unmistakable their lack of paternity becomes. It therefore follows that since males will be selected to avoid provisioning unrelated offspring, rather than attempt to elicit donations of meat from any male, irrespective of paternity, females will be selected to concentrate on eliciting donations of meat from males whose confidence of paternity is high.

The foregoing considerations suggest a very important conclusion concerning the likelihood of the evolution of monogamy as a dominant strategy, and the psychological mechanisms regulating sexual commitment which are discussed in the next chapter. The key point is that since males will be selected to demand confidence of paternity as a pre-requisite of sustained commitment, females should be selected to *enhance* any given male's confidence of paternity, even where, in reality, this has a less than advertised basis. From the point of view of any particular female, it would plainly be more advantageous to receive meat donations from more than one source, but only if the confidence of paternity of each donor could be guaranteed. Thus, a basic cleavage is suggested between the relative interests of males and females over the

matter of female sexual behaviour, in which *male choice* will constrain females to deceptively advertise commitment to any particular male.

From the point of view of the male, we have already seen that his commitment will be selected by both female choice and by direct benefits to his inclusive fitness conferred by making continued donations of meat to his offspring. It would therefore seem to follow that providing the male can display a convincingly acceptable degree of commitment to the female he is provisioning, and adequately provision his own offspring (relative to others in the population), his fitness would be best served by attempting to obtain additional matings. There is of course no necessary reason why we should expect males to be constrained by female choice to be monogamous; all female choice can be selected to demand (apart from successful insemination) is commitment to *resource provisioning*, and thus, provided the male's resources are adequate to support the offspring of more than one female with equal effectiveness, females could be selected to accept polygyny (the so-called *polygyny threshold*). Indeed it has been suggested that polygynous behaviour in males actually confers a selective advantage on females with whom they mate (Badcock 1990; Fox 1988). This is because males who possess genetic material underwriting the capacity for successful polygyny, will tend to transmit this tendency to their sons and thus the inclusive fitness of the female will be enhanced through the polygyny of their male offspring.

Certainly there is impressive evidence that the primal human mating system was polygyny. Badcock (1990), for example, summarizes important physiological adaptations - marked sexual dimorphism, sexual

bimaturism, differential life-expectancy and female menstrual synchrony - which strongly support a polygynous model. Nevertheless, there are a number of additional points which need to be discussed in order to clarify the exact nature of the sort of psychological mechanisms which this situation could have selected. First of all, although, as I have said, the principle interest of the female will be in the male's commitment to provisioning, it should not be forgotten that females will not be selected to stop at a single offspring. At the same time, since a female who has responsibility for the offspring of another male ought to be less attractive than one without such a responsibility, her best strategy will clearly be for her subsequent offspring to have the same father as the first. It therefore follows that females are likely to be selected to prefer males - polygynous or not - who will maintain a sustained commitment to *them*, rather than to any particular offspring they may produce. Of course, at first sight this does not seem to preclude the possibility of females tolerating *philandering* behaviour in males. This is because males who enhance their fitness exclusively through additional *mating*, rather than *parental* effort (that is, who merely inseminate additional females without provisioning them), do not detract from the availability of resources to the regular mate(s), or seem to pose any particular threat to the probability of subsequent fully provisioned pregnancy. Yet further reflection suggests that there may be a greater risks to regular mates than at first are suggested.

The main risk associated with the toleration of male philandering from the female point of view is that of the possibility of a subsequent transfer of commitment, that is parental *and* mating effort, to another female. Such a strategy would be selected in males if the offspring of the new

female were likely to be adaptively superior to any further offspring which might be produced by the original female. Since age is so crucial a factor in determining the reproductive success of any particular female, such a situation is entirely predictable, in fact largely inevitable. The point, therefore, is that a male who philanders with pregnancy as a consequence will be subject to selection pressures to provision this female in favour of his original mate (assuming of course that his resources are not infinite). Since there will be such strong selective pressures acting on males to maximize their fitness at the expense of their original mates by acting in this way, it is reasonable to conclude that equally strong pressures will exist for females to resist male philandering.

A further reason why philandering on the part of males might be expected to lead to the formation of new mating and parental commitments is the choice exercised by his female targets. Although it is possible to imagine situations in which it might pay a female to obtain copulations from males without subsequent provisioning - essentially when an existing male can be relied upon to provision any offspring this female may produce - such a scenario could not be expected to obtain in the majority of cases. This is because, first of all, since the male's target could be as yet unattached to a provisioning male, in the event of the female target offering greater reproductive potential than his existing mate, a transfer of commitment would be in his evolutionary interests. Secondly, however, we have already seen that female choice will be selected so that females prefer males displaying commitment, and so, just as with any other case, a philandering male would have to *display* commitment as a basic pre-requisite of a successful conquest. From the point of view of his original

mate, therefore, a male's philandering could be taken as a reliable indicator that he has at least *displayed* commitment to another female, and so that his commitment to her is in possible jeopardy.

Looked at from the perspective of his original mate, then, a male's sexual interest in other females must pose a threat in terms of continued provisioning and subsequent provisioned pregnancies. Whilst, as I have said, this scenario does not rule out polygyny as an adaptive mating strategy for the female, it does suggest that females should be selected to regard evidence of their mate's sexual preference for any other female or females with suspicion. It further suggests that since females stand to lose from this situation, they will be selected to respond appropriately: that is, by reducing their commitment to their partner accordingly. Likewise, since the adaptiveness of male provisioning is so crucially dependent upon confidence of paternity, males should be selected to regard any apparent sexual preference for other males in their mates with equal suspicion, and ultimately also reduce *their* commitment to their partner. It thus follows that since both males *and* females stand to lose the commitment of their partner in the event of their sexual preferences for another individuals becoming apparent, selection will strongly favour mechanisms in both males and females which serve to successfully advertise commitment, and disguise the formation of new sexual interests.

Of course it may be objected that this line of reasoning fails to take account of the fact that an individual choosing to remove all commitment to another in favour of an adaptively superior object, should not be selected

to take any interest at all in the sustained commitment of the *original* object, and therefore, that there should be no selection to deceptively advertise commitment to an object when none exists. This is an excellent point, and no doubt perfectly correct in instances where commitment has indeed been firmly re-established elsewhere. Nevertheless it has to be remembered that before such a situation can be reached, the actor in question has to establish that the potential object of commitment is indeed preferable to the existing object(s) in each and every respect. In other words, not merely do their superior parental characteristics need to be demonstrated - in terms of heritable adaptations and parental care - but also the likelihood of their maintaining sustained commitment. Thus, a female contemplating a new mate would have to be satisfied with his superior hunting prowess (as both a heritable characteristic and as a factor in his provisioning potential) and his likely degree of future commitment to her. A male, on the other hand, would need to establish the female's maternal potential - both physiologically and behaviourally - and, again, her likely degree of future commitment to him, as representative of his expected confidence of paternity. The effect of this situation, therefore, is that since these factors can only be established through relatively lengthy research, there will certainly be occasions - perhaps the majority - when a potential new object of commitment, chosen initially perhaps on the basis of some overt physical superiority, on the basis of subsequent investigation turns out to less appropriate than originally seemed to be the case. Consequently, the period of investigation between the initial arousal of interest, and the point at which the object is finally deemed to be superior in each of these respects, will be one in which it will be necessary to obscure all of this from the notice of the original mate. Thus, there will

always be a period where a deceptive display of commitment will be necessary so as to avoid, as it were, "burning one's boats" prematurely.

What this argument establishes, therefore, is that there will be selective pressures acting upon both males and females to *appear* more committed to their sexual partners than is in fact the case. But since all successful deception, by definition, benefits the deceiver at the expense of the deceived, there will also be selective pressures acting upon both males and females to *resist* the deceptions being practised on them by their sexual partners. This, as fully recognized by Trivers (see chapter 7 above), can only lead to the evolution of deceptive and counter-deceptive psychological mechanisms of increasing subtlety. It is to these mechanisms that I turn in the following chapter, and to the elucidation of what must so far appear to be the quite random connection between sexual variation and the evolutionary logic of sexual commitment.

CHAPTER NINE

Sexual Variation 2: The Evolutionary Psychology of Sexual Commitment

Before I move on to discuss the precise mechanisms involved in the advertisement of sexual commitment, and how these can shed light on apparently inexplicable variation in sexual behaviour, I wish to begin by considering certain general features of these mechanisms which are strongly suggested by the selective pressures outlined in the previous chapter. The first point which needs to be made in this connection, and one which is equally applicable to the evolution of all forms of deception, is that the rapid selection for increasing subtlety (Trivers 1981, 1985) is effectively equivalent to selection for increasing *plausibility*. To see why this is so, we need only notice that the most reliable indicator that another individual is cheating would be their displaying levels of altruism beyond that which could evolve. In other words, since it is a fact of nature that altruism (beyond the special cases of kinship and reciprocity already discussed) cannot evolve, it could be reliably predicted that displays of apparent altruism, not falling into these categories, could not be the product of evolution, and therefore, for the displays in question to have evolved, they must have conferred some benefit on their bearers; they must, in other words, be deceptive. It seems reasonable to surmise, therefore, that since (random mutations apart) apparently altruistic behaviour must in reality confer a benefit on the actor, individuals will be selected to treat such behaviour in others with suspicion. Consequently such overt displays of implausible "sham" altruism ought to be selected against as deceptive strategies, in favour of behaviour which *appears* to

confer a benefit on the actor, albeit in reality a greater benefit than is apparent.

This means, therefore, that in situations where the future conduct of others is crucial for the success of a planned course of action, it will always be in the evolutionary interests of any actor to believe the self-interested motives of others, whilst remaining sceptical about their claims to altruistic motivation. It follows, therefore, that those deceptive strategies which are likely to be selected in favour of displays of implausible altruism, will be those in which the actor stands to gain some obvious benefit, whilst ultimately gaining more than this: in other words, it will pay an actor, not to appear *more altruistic* than he actually is, but instead to appear *less selfish* .

If this reasoning is applied to the example of sexual commitment, it can be seen that given the fact that it will often pay to display greater commitment than in fact one has, such displays ought, in theory, to be selected to be based upon the apparent self-interest of the actor concerned. The difficulty with such an analysis is that, at first sight anyway, there does not appear to be any way in which individuals could be selected to regard displays of apparent commitment as plausible. This is because, as I have pointed out at length in the previous chapter, since it will always be in one's interests to appear to be more committed than in fact one is, any display of commitment should reliably indicate deception, and so selection should favour individuals who do not accept claims of commitment in others. Although this is a strong objection, it fails to consider two crucial points. First, both males and females stand to gain from practising a degree

of genuine commitment: females because as the possibility of successfully persuading other males of their confidence of paternity diminishes, so the risks associated with infidelity in terms of the loss of commitment from the original mate increases; males because their commitment benefits their own offspring through resource provisioning. The second point is that the degree of commitment it will in any particular instance be adaptive to render will vary with the characteristics of the partner. Thus, males will be selected to show a greater degree of *genuine* commitment towards females who possess valuable adaptive characteristics, and with whom their confidence of paternity is high, than females who lack such characteristics, or with whom their confidence of paternity is low. Correspondingly, females will be selected to show greater genuine commitment to males who are both in possession of adaptive characteristics, and whose own commitment to protracted provisioning is high. The significance of these points, therefore, that commitment can be both genuine and variable, is that it is after all possible for displays of commitment to be based upon self-interest, and hence for deceptive displays of commitment to successfully evolve.

Ambivalence and Sexual Object Choice

Although as I have just emphasised, selection would have favoured the evolution of genuine commitment in both males and females, the fact remains that it will always be adaptive to exaggerate the extent of this, so as to elicit greater levels of commitment from one's partner, than one's own commitment warrants. It is with this in mind that I now wish to consider one curious, but nevertheless central, theme in the classical

psychoanalytic theory of object-relations: ambivalence. According to psychoanalytic theory, a fundamental ambivalence of attitude is typical of close emotional relationships:

> The evidence of psychoanalysis shows that almost every intimate emotional relation between two people which lasts for some time - marriage, friendship, the relations between parents and children - contains a sediment of feelings of hostility, which only escapes perception as a result of repression (Freud, 1921, S.E.18, p. 101).

Crucial though this idea is, not merely to the psychoanalytic conception of "normal" relations, but also to the whole theory of psychopathology, it does appear, on the face of it, to be somewhat far-fetched and unnecessarily elaborate. If normal affectionate ties are only brought about by the repression of hostile wishes, which inevitably co-exist alongside them, the question is raised as to why it is necessary to postulate the existence of the hostile element at all, when exactly the same effect could be achieved by the assumption of the affectionate element alone.

Freud's own justification for this assumption was clinical, based upon the free associations of his patients, and the evident value of the concept in providing a satisfying metapsychological account of such psychopathological states as obsessional neurosis, paranoia and melancholia (or what we should today call depression). Considered in the light of earlier discussions, however, it is possible to see that a compelling evolutionary justification for the assumption of this phenomenon, emerges from the considerations detailed in the previous chapter. The essence of my argument about the evolution of commitment has been

that whilst it will be adaptive to advertise a greater level of commitment than in fact exists, selection should favour genuine commitment, as well as the ability to break this under circumstances where it will be more adaptive to do so. The notion of ambivalence, therefore - the co-existence of positive and negative feelings towards an object - would seem to correspond to this expectation: positive feelings reflect the genuine commitment to the object, whilst the negative feelings, the need to break the commitment. But exactly as evolutionary theory predicts, the two sides of the ambivalence are fundamentally contradictory; that is, because the effectiveness of the positive feelings in eliciting commitment from the object would be compromised by the simultaneous advertisement of their provisional nature, it will, according to my earlier discussion of self-deception, be maladaptive for both sides of the ambivalence to be simultaneously admissable to consciousness.

The clue to this can be found in Freud's mention of repression in the quotation given above. It will be recalled that acccording to the view of deception and self-deception I advanced in earlier chapters, repression is seen as a means by which self-serving wishes for which publicly acceptable motives cannot be formulated are kept from consciousness, and hence not enacted in that form. In the context of ambivalence, therefore, this understanding of repression seems well justified: the negative hostile wishes which co-exist alongside the positive ones serve the actor at the expense of the object, and so need, for their effectiveness, to be obscured from the object's awareness. The fundamentally genuine - albeit exaggerated - nature of the positive feelings, means that according to the considerations set out at the beginning of this chapter, they ought to be

effective in their deceptive deployment by virtue of their plausibility. In other words, the positive feelings should be *believed* by the object because they could be in the genuinely selfish interests of the actor. The fact, however, that the negative hostile feelings (that is, those antithetical to commitment) are *repressed*, means that they are, according to the psychoanalytic model discussed in earlier chapters, constantly seeking conscious representation, and indeed may break through into consciousness if sufficiently distorted to be acceptable to the demands of the super-ego, and yet still close enough to their original repressed root to be motivated.

Although the notion of ambivalence does indeed seem to be supported by the expectations of evolutionary theory, to see precisely how close the classical psychoanalytic model of object relations actually is to the evolutionary model outlined in the previous chapter, we need to consider the role played by the mechanisms of defence in maintaining the repression of the negative side of the ambivalent attitude towards the object.

Reaction-Formation, Projection and the Return of the Repressed

It is a central tenet of psychoanaytic theory that under circumstances where a straightforward repression does not suffice to keep an undesirable idea from consciousness, the ego may invoke additional defensive measures to ensure that the task of repression is successful[1]. One such

[1]The role played by the defences in the evolution of self-deception in reciprocal relationships is discussed by Alan Lloyd and Randolph Nesse (forthcoming, 1990).

mechanism, thought by Freud to be the principal mechanism responsible for the formation of obsessional neuroses, is reaction-formation. This defence seeks to avoid the conscious emergence of the unacceptable impulse by means of consciously and compulsively enacting its opposite (e.g S.E.20. p.102). In the present context, therefore, the repressed part of the ambivalent attitude towards the object - the negative hostile wishes representing a *lack* of commitment - would be kept from consciousness by means of compulsively exaggerating its opposite, that is, the positive, commitment- advertising attitude towards the object.

If we consider for a moment the circumstances likely to produce the deployment of defences, the assumption of the existence of this mechanism seems to be particularly well justified. According to psychoanaytic theory, repressed material is most likely to return in the presence of some event or circumstance in the real world which calls it forth. For example, following the above analysis of the repression of the negative side of an ambivalent attitude, it seems likely that this material will be called forth by the presence of a new object who has the potential to be adaptively superior to the current object. Of course, it will be completely in the interests of the actor for such feelings to exist, since, as we have seen, it may well ultimately prove adaptive to break his existing commitment in favour of its establishment elsewhere. However, until the viability of this is established, it is most important that such feelings are obscured from his current object. The problem therefore is that the actor in question must take an interest in the new object, whilst continuing to display his commitment to his present object. Since his interest will inevitably be in danger of being betrayed by subtle indicators -

the detection of which we can expect evolution to strongly favour - we can expect evolution to also favour means by which these indicators can be countered; that is, exaggerated displays of commitment at precisely the time when its bogus nature is most likely to be revealed. Seen in this light then, the emergence of a reaction formation against repressed hostile wishes which threaten to break through into consciousness is easily explicable as a compensation for the weakening of one's genuine commitment at a time when it would be adaptively harmful for this to be advertised.

Of course, this example also clearly illustrates the points made earlier about the role of consciousness in deceptive behaviour. It will be recalled that in my discussion of consciousness, repression and self-deception (chapter 6 above) I suggested that motives capable of conscious expression were likely to be those which meet the standards of public acceptability, understood as that which it will be in one's adaptive interests for others to be aware. In the case in question, this is clearly so. According to psychoanalytic theory, the subjective experience of an actor practising a reaction-formation against negative hostile feelings towards an object will be one of extreme tenderness and devotion, whilst the very thought of anything even loosely connected with the repressed wish will fill them with horror and disgust. In other words, the content of consciousness clearly corresponds to the interests of the object and whilst, as we have seen, the display must be plausible, it cannot be taken as representing the genetic interests of the actor in any obvious or straightforward way.

Another of the mechanisms of defence posited by psychoanalytic theory, which makes particular sense in the context of object-relations, is the mechanism of *projection*, and in particular its role in the dynamics of jealousy. Freud regarded projection - understood as the imputation of a repressed wish to an object - to be the primary defence mechanism operative in paranoia, including its manifestation in the form of pathological sexual jealousy (1922, S.E.18)[2]. Jealousy, according to this view, is the result of the imputation of one's own desire for infidelity to one's sexual partner. For Freud, this was explicable, as with all mechanisms of defence, as a means of alleviating intra-psychic conflict, which in proximate terms is no doubt correct. When considered in the light of evolutionary theory, however, it is clear that the mechanism in question is easily explicable as another subtle strategy for the deceptive advertisement of commitment.

To see why we should expect projected jealousy to be so successful a deceptive strategy, it is necessary to recall the important point made at the beginning of this chapter: *deceptive strategies will increasingly be selected by their essential plausibility*. Seen in this context, it is not difficult to see why jealousy should be plausible: displeasure at the infidelity of a partner to whom one has displayed sexual commitment is, as we have seen, entirely to be expected in a species such as ours in which male parental investment evidently played so important a part in its evolution. At first sight, however, it is not immediately clear why such easily explicable

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[2] In this paper,"Some Neurotic Mechanisms in Jealousy, Paranoia and Homosexuality", Freud distinguishes between two forms of pathological jealousy: projected and delusional, but since both rely on projection as the underlying mechanism, I shall ignore the distinction here.

feelings should be regarded as deceptive. After all, as I have just emphasised, jealousy seems to be a perfectly explicable reaction to discovering the fact that one has become the *victim* of deception, hardly a position from which one has anything to gain by acting deceptively. All of this is quite true, but in fact far from questioning the deceptive value of jealousy, explains why we should expect it to be so successful in facilitating deception.

To see how this is so, we need only remember what is in fact advertised by jealousy: disappointment that the object of one's commitment has not themself exhibited commitment. Yet the very fact that this object's commitment is deemed to be worth having, suggests that the jealous individual values their adaptive characteristics relatively highly; if they did not, then we should expect total indifference. As we have already seen, an indication that another individual regards one as worth investing in, is likely to suggest that their commitment can be counted on; and if their commitment can be counted on, other (adaptive) factors being equal, it will be advantageous to exhibit commitment to *them*, as opposed to someone whose commitment is in doubt. In this way, by appearing to be concerned about the commitment of one's partner, one is in fact advertising one's own commitment to them, and thereby increasing the chances of ultimately eliciting their commitment.

As with the case of reaction-formation, the mechanism of projection would be most likely to be invoked when the repressed desire is most in danger of breaking through into consciousness, that is, when the actor's lack of commitment is most likely to be revealed. But with far greater

subtlety than reaction-formation, projected jealousy appears to be so obviously selfish, that its deceptive nature could hardly be reliably predicted. Of course, if jealousy was only ever a product of projection we should be entirely justified in expecting selection to favour individuals who rejected such displays as deceptive. We have seen, however, that jealousy should be quite legitimately motivated in instances where the commitment of the partner is valued, and selection should therefore favour the interpretation of jealousy as an indicator of the jealous individual's commitment. In this way, projected jealousy ought to appear identical to "normal" jealousy, and so the ability to discriminate between the two will be correspondingly unlikely to evolve.

Guilt, Masochism and Deception

A difficult, but nonetheless important problem for psychoanalytic theory, has always been the general issue of self-injury, as manifested in such phenomena as guilt, depression and masochism. The theoretical difficulty which this phenomenon posed was that if, as Freud's theory claimed, human beings are motivated by the *pleasure principle*, whose aim is the satisfaction of biologically determined drives, motivated self-injury, of an often compulsive kind, could not be explained. In the context of modern evolutionary theory the difficulty is all the more pronounced: there does not seem to be any obvious explanation of how such behaviour could possibly improve an individual's inclusive fitness; indeed at first sight it looks as though it would have exactly the opposite effect. It is well known that this general problem led Freud to posit the existence of a "death instinct" (eg, "Beyond the Pleasure Principle" 1920, S.E.18, pp.1 - 64), which

although taken seriously by a number of post-Freudian writers (most notably Herbert Marcuse), has, as Badcock has correctly pointed out, no place in modern evolutionary biology (Badcock 1986, 1988). In view of the argument I have expressed so far, however, the problem posed both for psychoanalytic and evolutionary theory may not be as serious as at first it seems, and would seem to lend additional support to the analysis of commitment and deception so far developed.

Taking the issue of guilt first, it is helpful to begin by considering the dynamics of guilt as understood by psychoanalytic theory. Freud held that:

> An interpretation of the normal, conscious sense of guilt (conscience) presents no difficulties; it is based on the tension between the ego and the ego-ideal and it is the expression of the condemnation of the ego by its critical agency (1923, S.E.19 pp.50-51).

Following my earlier discussion of the significance of the super-ego, this analysis seems entirely plausible. If, as I have suggested, the super-ego is understood to be the internalized standard of public acceptability, its opposition to wishes which act against the interests of those it represents, could reasonably be seen as a safeguard against their reproaches, which, as discussed earlier, could outweigh the benefits of the enactment of any such wishes by the withdrawal of their commitment. To fully comprehend the significance of this observation, however, it is necessary to bring into consideration Freud's relatively neglected concept of *idealization*, or *over-valuation of the object*. He describes this phenomenon as follows:

> Contemporaneously with this "devotion" of the ego to the object, which is no longer to be distinguished from a sublimated devotion to an abstract idea, the functions allotted to the ego-idea entirely cease to operate. The criticism exercised by that agency is silent; everything that the object does and asks for is right and blameless. Conscience has no application to anything that is done for the sake of the object; in the blindness of love remorselessness is carried to the pitch of crime. The whole situation can be completely summarized in a formula: *The object has been put in the place of the ego ideal* (1921, S.E. 18,p.113, *Freud's italics*)

The notion that under such circumstances the critical functions of the ego-ideal are ceded to the object quite evidently corresponds to the above analysis of guilt: it is not merely some generalized standard of public acceptability, based upon parental prototypes which regulates that which is admitted to consciousness, but the interests of precisely the individual whose acceptance the actor's actions must obtain if they are to succeed. Thus unconscious wishes which are likely to contravene the interests of the object - because they indicate a lack of commitment - will, if there is a threat that they will be aroused by external events, be met with guilt; and guilt, whilst a subjectively unpleasant emotion, is significantly less harmful to fitness than the condemnation of the object which would result were the desire or action which elicited the guilt enacted, or its enactment continued. In other words, the concept of idealization and its role in this analysis of guilt are explicable as a mechanism of pre-emption: idealization, which must be based upon a genuinely high evaluation of the object, means that actions which would be likely to lead to the abandonment of the object's commitment are effectively made a source of

unpleasure for the actor, and are therefore likely to be at least temporarily suspended.

Where Freud's theory of guilt is again particularly interesting is in the fact that it explains what might be regarded as both *rational* and *irrational* guilt. By this I mean that it is able to explain not only instances of guilt which follow conscious actions or wishes on the part of the actor, but also those which do not seem to have any cause, at least of which the actor is consciously aware. Freud frequently wrote of an "unconscious sense of guilt" (e.g. 1923, S.E.19, pp.49-54; 1924, S.E. 19, p.166), which although some may find paradoxical, refers merely to the fact that a tension between the ego and super-ego need not be conscious; an observation which the arguments I have advanced so far strongly endorses. Indeed the distinction - which has obvious empirical support in both clinical and everyday settings - seems to lend further support to the suggestions I have already made about the deceptive deployment of cues of commitment. In situations where guilt is elicited by unconscious wishes (and not overt actions), the absence of the actor's awareness of its source will obviously increase the chances of keeping its source from the object of commitment. Under circumstances, however, when guilt follows the performance of actions which obviously demonstrate the weakness of the actor's commitment to his object, an awareness of its source will be advantageous to the actor for the following reason: The action in question - an act of infidelity for example - does not necessarily imply the *inevitability* of the actor's transfer of commitment. We saw in the previous chapter that there will be a period of time during which an actor, contemplating the transfer of commitment to a new object, will be uncertain of the viability -

in terms of the new object's commitment - of the transfer. This suggests that the actor's initial interest in some superior characteristic of the new object could go either way: either the object proves adaptively suitable for a transfer of commitment, or they do not. In the first instance, we should expect the commitment to the original object to be broken, and, according to the above argument, the new object to be idealized. In the second instance, however, the implication is that the original object is judged to be superior to the new rival after all, and under such circumstances, it is quite plausible for the actor to continue his commitment to the original object. Since a sense of guilt indicates that the object is after all valued - because their reproaches, and implied withdrawal of commitment is implicitly deemed to be undesirable and hence maladaptive for the actor - its display, following an act of infidelity which could be discovered, and which did not lead to a transfer of commitment, would therefore be in the evolutionary interests of the actor.

This latter point leads us conveniently into the consideration of another, closely related issue: Freud's analysis of melancholia. For Freud, feelings of melancholia - or in modern terms *depression* - are explicable as the turning of an unconscious hostile wish towards an object, back onto the subject's own ego. Thus he writes:

> ...the self reproaches are reproaches against a loved object which have been shifted away from it onto the patient's own ego...(1915d. S.E. 14, p248).

Thus:

> The woman who loudly pities her husband for being tied to such an incapable wife as herself is really accusing her *husband* of being incapable, in whatever sense she may mean this (Ibid, *Freud's italics*).

Although this picture of the dynamics of melancholia seems partly justified in that it at least prevents negative feelings towards an object from becoming manifest, the question of why they should be turned back onto the self remains open. After all, if the actor's claims to general inadequacy and unworthiness are convincing, then there seems to be the very real possibility that they will convince the object that this poor individual really is as inadequate as they claim, and hence quite unworthy of their commitment.

In answer to this, however, we must first of all consider the effects of a statement of self-deprecation from the point of view of the actor's sexual object. An issue which has run more or less implicitly throughout the discussion so far has been the question of *objective sexual value*. By this I mean that it follows with inescapable obviousness from evolutionary theory that some physical and behavioural characteristics will be, in their objective consequences, more evolutionarily successful than others, and so evolution will favour individuals who prefer to form sexual relationships with individuals in possession of these characteristics. Now whilst, as I have already emphasised, and as critics of sociobiology never seem to tire of pointing out, this fact does not, on the face of it, seem to square with the facts of sexual variation, as a basic principle of evolution it remains unassailable; writers such as Jeffrey Weeks (1986) who deny it as a

matter of course, are simply ignorant of evolution[3]. It follows, therefore, of necessity, that the commitment of some individuals will be more desirable to more people than that of others. This suggests that each individual should face a trade-off between the desirability of a partner, and their expected level of commitment; in other words, since more desirable individuals will have greater opportunity for adaptive transfers of commitment, their commitment is less reliable than that of less desirable individuals. This point has one important consequence for the present argument: individuals who have good reason to believe that their partner regards themself as less desirable than is in fact the case, can reliably predict a high level of commitment from that partner.

Seen in this light, the exaggeration of the desirability of a partner which results from the sort of self-deprecation we are discussing, can easily be regarded as another strategy for the exaggeration of one's own commitment. It is again particularly relevant that Freud saw the cause of the phenomenon in the unconscious attitude of the actor towards the object; the response to the pressure of this attitude is, once again, to advertise the very thing its existence indicates: a lack of commitment.

Perhaps the most extreme of illustration of this problem of self-injury is the phenomenon of sado-masochism as a sexual perversion. Like melancholia and guilt, Freud regarded the masochistic element of the orientation as the turning back of a sadistic wish towards an object, onto the subject:

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[3] In fact, judging from this work, evolution is not all of which Weeks is ignorant; but that is perhaps another matter.

> It can often be shown that masochism is nothing more than an extension of sadism turned round upon the subject's own self, which thus, to begin with, takes the place of the sexual object (1905, S.E.7, p.158).

This conclusion is given further support when we consider Freud's observation that:

> ...the most remarkable feature of this perversion is that its active and passive forms are habitually found together in the same individual. A person who feels pleasure in producing pain in someone else in a sexual relationship is also capable of enjoying as a pleasure any pain which he may himself derive from sexual relations (Idid., p.159).

In view of this observation and the foregoing discussion, the explanation of this problem may not be hard to find. Quite clearly, sexual sadism is a desire to completely impose one's will upon the sexual behaviour of the object, and as such, in the light of the discussion so far, it seems justifiable to regard it as a desire to control the *sexual commitment* of the object. It may well be, therefore, that a positive attitude towards a degree of sadism in an object would evolve, since a desire to control the commitment of an object would, for the reasons already given, indicate the potential commitment of the actor. Of course the likelihood of any individual being selected to yield to the sadism of just *anyone*, is very small, since, as we have seen, commitment alone should not be a sufficient basis on which to choose a sexual object. Nevertheless, an object who displays the necessary attributes of desirability, together with an unmistakable desire to

control one's commitment, could well be a better evolutionary choice than one without such a desire.

Quite clearly, the potential commitment indicated by such a sadistic attitude would not be actualized unless the sadistic individual could guarantee the commitment of his object. This could be achieved in two ways: either the sadist maintains such powerful physical control over his object that transgression on their part is rendered physically impossible, or the object of the sadism voluntarily and preferentially submits to the sexual control of the sadist. Clearly the first possibility is highly unlikely, since the sheer effort involved in maintaining this level of control would effectively preclude any other activities, especially hunting. The second possibility, however, seems more plausible, since it would clearly demonstrate a selfishly motivated desire to be sexually dominated by the individual in question, and therefore plausibly advertise sexual commitment. Certainly, seen in this way, Freud's observation that sadism and masochism are frequently found in the same individuals, and his assertion that they share a common unconscious origin, seem quite realistic: both sadism and masochism, although apparently quite different, can be seen to advertise the same thing, that is, commitment.

It would of course be misleading to assume that sexual practices which can be reasonably considered sado-masochistic are confined to the well-documented practices of physical restraint and bondage. As two modern commentators point out:

> Alternatively...no physical or mental derogation or attack is carried out: instead lovemaking is

> carried out via a stylized ritual of commands and obeisances in which the enjoyment of one partner is at least in theory assured because he or she gets exactly what is desired, whilst the other partner achieves pleasure by fulfilling those desires (Gosselin & Wilson, 1984, p.95).

This observation clearly locates the more extreme and violent manifestations of sado-masochistic behaviour in relation to evolutionary hypothesis advanced above. Its emphasis on the *sexual pleasure of the dominant partner,* as a source of pleasure for the masochistic actor, is particularly revealing since it highlights the essentially intrerpersonal nature of the passive attitude. Of course at first sight such a concern with the pleasure of another individual may seem to be an example of altruism *par excellence;* yet if my argument so far is accepted, it can clearly be seen as an advertisement of concern over the commitment of the partner, and thus as a plausible advertisement of one's own commitment.

Also of significance to this argument is the fact that in the early editions of his Three Essays on The Theory of Sexuality, Freud attributed masochism to the phenomenon mentioned above, of sexual overvaluation of the object:

> One at least of the roots of masochism can be inferred with equal certainty. It arises from sexual overvaluation as a necessary psychical consequence of the choice of sexual object (1905, S.E.7, p.158).

This is important in two respects: first because it sees masochism in relation to an object which has been "overvalued" and so, in terms of my argument, one whose commitment the actor is interested in eliciting;

secondly, however, because this overvaluation is seen as a *necessary* psychical consequence of object choice. If it is indeed the case that a degree of overvaluation can normally be expected to accompany object-choice, the assumption that this phenomenon is related to the advertisement of commitment seems to be well justified. This is particularly clear when we consider:

> It is only in the rarest instances that the psychical valuation that is set on the sexual object, as being the goal of the sexual instinct stops short at its genitals. The appreciation extends to the whole body of the sexual object and tends to involve every sensation derived from it. The same over-valuation spreads over into the psychological sphere: the subject becomes, as it were, intellectually infatuated (that is his powers of judgement are weakened) by the mental achievements and perfections of the sexual object and he submits to the latter's judgement with credulity. Thus the credulity of love becomes an important, if not the most fundamental source of *authority* (Idib. p.150, *Freud's italics*).

The reason why this extension of interest beyond the genitals of the object should be selected as an indicator of commitment is not hard to see. Whilst genitalia are of obvious importance in determining the evolutionary success of an individual, they are nevertheless a necessary rather than a sufficient condition of that success. Thus, the most evolutionarily successful object-choices will be those which take factors besides the genitals of the object into account. Specifically, they will be choices based upon such crucial adaptive criteria as parental potential and likely commitment, but also on general physical and intellectual condition. It would thus seem likely that since anyone interested in

making a commitment to an object will be selected to choose that object on the basis of such criteria, an individual in whom such an interest is being taken might be selected to interpret that interest as an indicator of the potential commitment of the interested individual. Therefore, an individual who demonstrates a positive reaction to such features in another could reasonably be expected to provide the necessary commitment in the future, and might thus be expected to elicit a positive response in terms of commitment from that object. If this is correct, then it seems reasonable to surmise that a mechanism by which commitment is advertised would be one which emphasised the actor's satisfaction with these general fitness-related features, with greater levels of satisfaction indicating greater levels of commitment.

In this light, then, the possibility, suggested by the concept of overvaluation, that sexual desire might become attached to parts of the object not directly related to reproduction begins to seem plausible. Sexual arousal at the sight of an individual's eyes, lips, hair, indeed any part of their body, could clearly be selected if these features reliably indicate fitness. Yet at the same time, since this arousal will also indicate likely commitment, it will be in the interests of the aroused individual to exaggerate their evaluation of these characteristics, so as to deceptively advertise their future commitment. This observation may go some way to explaining Freud's contention that overvaluation of the object is invariably accompanied by a decrease in the specifically genital orientation towards the object. This is because a single-mindedly genital orientation to an object would indicate a general indifference to the fitness qualities of the object, and thus suggest a lack of future commitment. If however, the

desire for genital union - which will obviously have to follow - is kept to a minimum, in favour of a desire for the more generally adaptive characteristics, future commitment is indicated.

It is with the foregoing discussion in mind that I wish finally to return to a consideration of some of the apparent paradoxes in human sexual behaviour which were alluded to at the beginning of the previous chapter, and drawing on the theoretical argument with which this thesis has been mainly concerned, offer some tentative answers to these problems.

Variation, Commitment and Deception: Some Conclusions

It will be recalled that the essence of the problem which was raised earlier concerning variations in human sexual behaviour, is one of explaining the existence of subjective preferences for sexual activities and objects which are either less adaptive than they could be, or in some cases positively devoid of any adaptive value. What I have tried to suggest in the course of this and the preceeding chapter is that in view of the arguments advanced earlier, we should subject to scrutiny the evolutionary significance of the psychological mechanisms underlying subjective sexual preference itself, and not merely attempt to find some "adaptationist" explanation for such behaviour. In this connection I have suggested that a decisive aspect of human sexual relations will be commitment, and therefore that we can expect the evolution of complex psychological mechanisms mediating the deceptive advertisement of commitment and the corresponding detection of this deception. If we

now apply these arguments to the problem in hand, a number of difficulties can be resolved.

One important example of such problematic behaviour which I raised earlier is sexual fetishism. Seen in the light of the above discussion of the Freudian concept of sexual overvaluation as a strategy for the deceptive advertisement of commitment, however, it is possible to see that it is not as paradoxical as at first it seems. Freud wrote of fetishism:

> Some degree of diminution in the urge towards the normal sexual aim...seems to be a necessary precondition in every case. The point of contact with the normal is provided by the psychologically essential overvaluation of the sexual object, which inevitably extends to everything associated with it. A certain degree of fetishism is thus habitually present in normal love, especially in those stages of it in which the normal sexual aim seems unattainable or its fulfillment prevented (1905, S.E.7, p. 153-4).[4]

The idea that fetishism might be related to a decrease in normal sexual (genital) interest, is given further empirical support by North's analysis:

> Normal sexual intercourse may be unsatisfactory to him [ie. the fetishist] unless accompanied by additional acts. Idiosyncratic genital activity or activity seemingly remote from genital intercourse may thus be more satisfying to him (North 1970, p.42).

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[4]Freud goes on to quote Goethe:

Get me a kerchief from her breast
A garter that her knee has pressed

In terms of the argument advanced here, then, fetishism might be seen as the outcome of a mechanism for attaching sexual desire to fitness-related (that is, not specifically *genital*) aspects of the object, in the interests of displaying commitment. This suggestion may, in addition, shed some light on what at first sight is generally regarded as one of Freud's more far-fetched clinical observations:

> In every instance, the meaning and the purpose of the fetish turned out, in analysis, to be the same. It revealed itself so naturally and seemed to me so compelling that I am prepared to expect the same solution in all cases of fetishism. When now I announce that the fetish is a substitute for the penis, I shall certainly create disppointment; so I hasten to add that it is not a substitute for any chance penis, but for a particular and quite special penis that had been extremely imposrtant in early childhood but had later been lost. That is to say, it should normally have been given up, but the fetish is precisely designed to preserve it from extinction. To put it more plainly: the fetish is a substitute for the woman's (the mother's) penis that the little boy once believed in and - for reasons familiar to us - does not want to give up (1927, S.E. 21, p.152-3).

Now whilst the details of this assertion may be open to further debate, the fundamental observation that the fetish object is a gential substitute seems to be exactly what the above analysis suggests. The point is that commitment is advertised by an interest in parts of the body *other than the genitals:* in other words genital substitutes. Of course, the examples of fetish objects mentioned earlier - rubber and P.V.C. clothing, wet canvass shoes, lingerie and so on - could hardly be regarded as fitness-related aspects of the object; the very opposite in fact. But it should be remembered, as I have emphasised at length in previous chapters, that

since these objects would not have been present in the environment in which the psychological mechanisms in question evolved, there is no reason to expect the evolution of the means to distinguish between these and those which would be adaptive. Indeed, in his analysis of fetishism, Freud is quite explicit about the exact source of the *ontogenetic* emergence of an individual's particular choice of fetish object:

> One would expect that the organs or objects chosen as substitutes for the absent female phallus would be such as appear as symbols of the penis in other connections as well. This may happen often enough, but is certainly not a deciding factor. It seems rather that when the fetish is instituted some process occurs which reminds one of the stopping of memory in traumatic amnesia. As in this latter case, the subject's interest comes to a halt half-way, as it were; it is as though the last impression before the uncanny and traumatic one is retained as a fetish. Thus the foot or shoe owes its preference as a fetish - or part of it - to the circumstance that the inquisitive bot peered at the woman's genitals from below, from her legs up; fur and velvet - as has long been suspected - are a fixation of the sight of the pubic hair, which should have been followed by the longed-for sight of the female member; pieces of underclothing, which are so often chosen as a fetish, crystallize the moment of undressing, the last moment in which the woman could still be regarded as phallic (Ibid., p. 155).

In this quotation we see a particularly striking example of Freud's understanding of the relationship between phylogeny and ontogeny, which clearly illustrates the earlier theoretical discussion of the possibility of maladaptive behviour motivated in accordance with inclusive fitness demands. Here Freud is saying that the fetish object actually chosen, is

selected on the basis of two criteria: the phylogenetic tendency to choose a genital substitute as a fetish object, and the ontogenetic selection of a particular substitute on the basis of certain experiences. In this way, we have a very good example of the effect of evolutionary time-lags which was discussed in detail earlier: genital substitutes chosen according to the ontogenetic criteria given in the quotation from Freud above, would, in our evolutionary environment have been adaptively relevant, whereas those chosen in today's environment on the same basis would not. The *motivation* of the action is thus explicable, as the earlier theoretical discussion insisted it must be, as evolutionarily determined in accordance with inclusive fitness theory, whilst the *outcome* of the action is recognised as maladaptive, and this is explained by environmental change.

I wish now to broaden this discussion to consider the implications of the arguments advanced so far for wider issue of "cultural" variation in ideals of sexual attractiveness, which, as I pointed out earlier, is often taken by supporters of conventional sociological approaches as being a prime illustration of the biology-independence of human motivation. Considered in the light of the view of human consciousness which I have advanced, however, this analysis can be seen to be seriously flawed. The main difficulty which can be identified with this approach is its reliance upon the conscious, subjective preferences reported by actors: since actors appear, systematically, to prefer to copulate with objects who diverge from the expected standard of universal biological attractiveness, factors other than biology - namely *culture* - must be responsible for these preferences. Yet, as we have seen, from an evolutionary point of view, there is

absolutely no reason why we should expect consciously held preferences to correspond in any straightforward way to ultimate biological interests. Indeed, in view of the importance which I have suggested must be placed on the role of self-deception in human interaction, quite the reverse should be expected.

To see exactly how this can explain the problem of variation in ideals of sexual attractiveness it is necessary to begin by repeating the point, made in the previous chapter, that ideals of sexual attractiveness need to be relative to the objects available. In other words, since any actor will only encounter a very small number of sexual objects both worthy of their commitment, and who themseleves display a suitable level of commitment towards the actor, it will not be in their interests to be excessively discriminating. Yet where the foregoing argument greatly extends the importance of this somewhat self-evident observation, is in the importance given to the *advertisement* of commitment as a means of eliciting commitment in an object. As I argued earlier, since the commitment of the object will always be a crucial determinant of the evolutionary success of an actor, then it follows that even where the available object does not meet the theoretical adaptive ideal, the actor should still be concerned to elicit their commitment. This suggests therefore, that in the event of a less that ideal object being all that is available to an actor, it will still be in their interests to display their own commitment, and hence to mobilise the psychological mechanisms which have been discussed earlier in this chapter. Thus, if we take the mechanism of overvaluation, or idealisation, which, it will be recalled, I suggested could be explained as a means of displaying a deceptively high

level of commitment to an object, it is possible to see that the attribution of ideal qualities to a variety of different objects reflects not the genuine biologically determined subjective preference that the sociological critics would wish to believe, but rather the evolutionarily determined need to advertise commitment to any object - even those which are less than ideal - whose commitment it is in one's interests to elicit.

Looked at in this way, then, the apparent problem posed by an individual subjectively preferring a sexual object which departs from the theoretical ideal, over and above one which comes closer to that ideal, can be plausibly explained by the fundamental trade-off, referred to earlier, between commitment and evolutionary desirability. According to this argument, since desirability and commitment can be expected to be negatively correlated (because the more objectively desirable an individual - in evolutionary terms - the greater choice of sexual objects they must have) there must exist for each individual an optimum point for their sexual object on the commitment/desirability gradient. Since this will vary from individual to individual, depending upon their own possession of desirable characteristics, it follows that their choice of sexual object should also vary accordingly. As, therefore, we can expect this systematic variation in the desirability of objects from whom different individuals can be expected to elicit commitment, we can, in view of the need to advertise commitment, also expect a systematic variation in reported sexual ideals.[5]

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[5]Although necessarily speculative, it does not seem unreasonable to suggest that Freud's contention that parental prototypes form the basis of adult object-choices, might be explicable in the light of this. If, as we must assume, there is ultimately a genetic basis to the possession of desirable characteristics, then one's own parents could also be expected, in general, to possess these characteristics. It may be, therefore, that the most reliable

I have tried in this chapter to achieve two things: first and foremost I have tried to illustrate the argument, advanced in the course of earlier chapters, that evolutionary theory demands that we take an evolutionary psychological approach to the analysis of apparently maladaptive behaviour; secondly, however, I have tried to advance an analysis along these lines of some quite specific aspects of human behaviour. Whilst it is my belief that this analysis does offer a contribution to the substantive issues in question, in the present context it is intended mainly as an exemplification of the theoretical arguments advanced earlier. In the conclusion which follows, therefore, I wish to draw together the various strands of the earlier theoretical discussions, and shall use the examples elaborated here as illsutrations of my conclusions.

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indicator one has of the desirability one can reasonably expect in an object, is the desirability of one's parent of the opposite sex.

CONCLUSION

The central point which it has been my purpose to make in this thesis is that the assumption of culture as an independent source of human motivation which underlies conventional social theory cannot be sustained in the light of modern evolutionary knowledge. I have tried to demonstrate that instead, human action can only be legitimately understood as motivated biologically, and in accordance with the expectations of inclusive fitness theory. The chief difficulty which I have suggested exists with this necessarily adopted position, is that it appears not to be able to explain behavioural variation, and in particular maladaptive variation. My suggestion has been that this paradox is essentially the result of confusion over the level at which the evolutionary analysis of behaviour should proceed: rather than treat the currently observed behaviour of the organism as necessarily displaying adaptive significance - as is generally done by human sociobiologists - it is in fact necessary only to find an adaptive significance for behaviour which the mechanisms underlying the currently observed behaviour *would* have produced in the environment in which they evolved.

My argument has been that this observation - which is actually fundamental to the evolutionary analysis of the characteristics of *any* organism - suggests a very specific model of human behaviour which is in essential respects different from that commonly assumed by social theorists. To summarize the argument which I have developed to this end, it is helpful to emphasise the implications of this approach for the analysis of *meaning*, and its involvement in specifically human conduct.

As I have already pointed out (see especially Ch.3 above) the apparently uniquely meaningful nature of human conduct is generally taken as the central justification for the separation of human behaviour from the sphere of biology. Underlying this approach to the meaning of human action is the fundamental assumption that the relationship between human symbols and that which they signify is essentially arbitrary; that is, human action beyond gross non-conscious biological functions is significant, and hence explicable, to both actor and observer (and so is also thereby motivated) entirely by dint of convention. I have I hope demonstrated, in the preceding pages, that this conception of meaning is not merely internally incoherent in its own terms, but more conclusively can be shown to be utterly fallacious in the light of modern evolutionary knowledge.

What instead is suggested by the analysis I have pursued, and which is summarized briefly above, is that human symbols can never be considered arbitrary in this sense, but must rather be motivated in accordance with the principles of evolution; that is, the inclusive fitness of the organism. The implication of this is that according to evolutionary theory, as with conventional social theory, human actions must be considered *meaningful*, but that in the case of the former the meaning of actions must be considered evolutionarily relevant. Of central importance, however, is the point, made at length in earlier chapters, that the evolutionary meaning of an action need not be - and indeed often is not - identical to its current evolutionary consequences. Of course, often these two will coincide; but equally, they often will not. The crucial point, however, is that evolutionary analysis forces us to the conclusion that

whilst the manifest content of human actions are variable, with the effect that so also are their current adaptive values, their underlying meanings remain fixed, and *their* content explicable in terms of inclusive fitness theory.

It has been my intention, following this observation, to explore the relationship - revealed as crucial by this argument - between the evolutionarily determined deep content of human actions, and their often paradoxical, apparently maladaptive manifest content. In the course of this exploration, I have suggested that the theory of human mind and behaviour which comes closest to meeting the requirement of recognizing this relationship is classical psychoanalysis. My reasons for reaching this conclusion may be summarized by the following points: First, Freud recognized that the current, conscious contents of the human mind, and the actions to which these give rise, are in fact transformed representations of some other unconscious motive. In this way the need for a theory of the fixed nature of the deep contents of the mind is met. Yet where Freud's formulation of this relationship differs from those advanced by so many of his followers, is in its explicit recognition that the fixed deep content in question is genuinely biologically determined by the evolutionary process. Secondly, however, Freud's analysis attempts to explicitly analyse the paths along which the fixed biologically determined motives are transformed, as themselves biologically determined. Although, as I have pointed out, Freud's own evolutionary biological conclusions were incorrect, his insistence upon seeking ultimate evolutionary explanations for mental contents and processes makes the structure of his model uniquely relevant to the issues now raised by

modern evolutionary theory. Thirdly, the fundamentally evolutionary nature of Freud's thought led to a conception of the relationship between internally originating instincts and objects in the external world which is thoroughly compatible with modern evolutionary knowledge, and in fact, as I hope I have shown, provides the form of a satisfactory solution to the difficult theoretical problem of behavioural variation.

I have of necessity developed my argument at a high level of abstraction; no doubt this will have lead, at times, to some difficulty for the reader in conceptualising the concrete implications of what I have been saying. Hopefully, however, the examples I have elaborated in the last two chapters will do something to remedy this. To illustrate this, it is perhaps worth devoting some time to the further elucidation of my argument in terms of these examples.

Let us begin with the sense in which, for conventional social theory, an individual's choice of sexual object is considered meaningful.[1] For social scientists, human actions are considered meaningful in the sense that their contents are explicable with reference to some theoretical reality other than the action itself: actions, like words, *refer* to something which, by definition, they are not, and like words are connected to these *others* in a purely arbitrary fashion. Therefore, an individual's choice of sexual object is considered by this approach to be arbitrary in the sense that there

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[1] Of course, as I explained in chapter three, I concentrate on the respects in which conventional social *theories* in general differ from the evolutionary biological approach, and am thus ignoring what are usually regarded as fundamental differences between them; the point I am making, therefore, is, I would claim, equally applicable to all such approaches.

is no necessary connection between it, and the other reality to which it refers. It is also meaningful because it *does* refer to another reality which it is not. Thus for example, feminists may - and in fact some do - argue that the fact that the majority of human beings perform sexual acts with members of the opposite sex, is first of all an arbitrary *sign* - they could just as well copulate with members of their own sex, different species, or even, logically, not perform sexual acts at all - and secondly that this perfectly arbitrary sign in fact signifies some other reality - the patriarchal infrastructure of society, culture, or some other abstract construct. What is however denied, is that there is any respect in which the choice of sexual object is necessary; and therefore that there is any limit to the range of possible objects chosen, and actions performed with them. Indeed the regularity with which phrases such as "social definition" and "cultural construction" are used in social scientific "discourse", attests to the vast underestimation of the extent to which these fundamental assumptions can be said to constitute a sociological "paradigm". [2]

Another way of putting this argument is to say that for the conventional social theorist, those things which we call "sexual acts" are not so called - and experienced - as a result of anything intrinsic to the acts, but as a result of the arbitrary ascription of an externally determined significance. Of course different schools of social thought would advance very different explanations of the origins of the particular meanings which it is considered are so ascribed, as well as projections of their likely future

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[2]The degree to which these common features rest upon the naive assumption of an uncomplicated unidirectional process of "socialization" is of course criticized in Wrong's well-known paper (1964); although this paper is usually regarded as a classic, it is surprising how limited have been its consequences.

changes; the point however is that the definition of an act as sexual is something which is crucially dependent upon *other* antecedent arbitrary definitions. As Weeks has summarized this argument:

> ..we must learn to see that sexuality is something which society produces in complex ways. It is a result of diverse social practices that give meaning to human activities, of social definitions and self-definitions, of struggles between those who have power to define and regulate, and those who resist, Sexuality is not given, it is a product of negotiation, struggle and human agency (Weeks, 1986,p.25).

My argument, however, following the central insights of evolutionary biology, is that the meaning of sexual acts - and therefore their explanation- cannot be held to lie in the collective *sui generis* realm of the social. Sexual objects are not chosen because culture has drawn a line of demarcation between the sexual and the non-sexual and ascribed some objects a sexual significance and others not; objects are chosen and given a sexual significnce because evolution has selected certain specific behavioural patterns and not others, and objects of certain characteristics have been selected as the objects of sexual arousal. Where this argument is crucially different from that advanced by sociobiologists, however, is in the recognition that the significance of currently observed sexual objects - their meaning - whilst ultimately explicable as adaptive, need not currently be observed in the consequences of choosing such objects. For instance, a sociobiological explanation of the problem of sexual fetishism would need to find some current adaptive value in the practice; something which, for reasons detailed earlier, seems unlikely. According to the argument I have advanced, however, fetshism is explicable as a

means for the advertisement of sexual interest in parts of the object which would indicate fitness, and hence is a means for advertising sexual commitment to the object; this is the *meaning* of the fetish. I pointed out however that the presence today of objects which were absent in the environment in which we evolved, means that the objects actually chosen today need not have adaptive consequences. Yet the central point here, is that the meaning of the object remains the same, despite its lack of actual adaptive significance; the object is not arbitrary, has a precise meaning and is selected because of its possession of certain characteristics which would have indicated adaptiveness in our primal environment, but which do not today.

A criticism which has often been made of sociobiology is that it claims the course of human action is rigidly determined by biology, and hence contains within it a deeply pessimistic message for the future of human societies. I pointed out in chapter two that such judgements about the desirability of the consequences of scientific observations had absolutely no place in the assessment of the validity of such observations; nevertheless, this is certainly not to deny that one should be concerned about such consequences. On the contrary, one should take them very seriously indeed. It should be clear therefore, that the implications of pessimism often attributed to sociobiology could hardly be applied to the approach I have developed in these pages. This is because although I have insisted, in keeping with modern evolutionary theory, that human action must be motivated in accordance with the inclusive fitness interests of the individual, we have seen that the consequences of these actions can be very different from those we must assume were produced under primal

conditions. I am not of course suggesting that human behavioural variety is limitless - this would obviously contradict the entire argument so far - but merely that as environments change, so must the consequences of our biologically determined motivations. Precisely how this might happen is clearly impossible to say, but the presence of so much apparently altruistic behaviour - so readily assumed by social scientists to contradict evolutionary insights and prove the benevolence of culture - suggests that, *in spite of evolution,* things might not ultimately turn out as badly as some would suppose.

I hope readers of this thesis will be convinced by the solution to he problems raised by evolutionary biology for social theory I have offered. I hope also that the specific analyses of the examples considered in the final two chapters will prove of some value to the understanding of these questions; it seems to me that a consideration of deception and the advertisement of commitment must provide much of the solution to the paradoxes of human sexual behaviour. My principal hope, however, is that I have succeeded in demonstrating that the central assumptions of modern social theory cannot be sustained when the realities of biological evolution are considered, and that the challenge posed to social science by these realities demands a solution of a genuinely radical kind. If I have done so, then it is my further hope that the work contained in this thesis, and that which will follow it, may make some contribution to this end.

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